Hans Jürgen Holstein

HOMO CYBERNETICUS 2

Towards an Epistemology of Dynamic Causal Modelling

UPPSALA: SOCIOGRAFICA. 1974

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"Mankind is continuously threatened by two things: Chaos and Order."

Pierre Schaeffer

"Without deviance there is no progress."

Frank Zappa

Hans Jürgen Holstein

Homo Cyberneticus

Uppsala: Sociografica 1974

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Printed in Sweden by
TEXTGRUPPEN I UPPSALA AB

Preface

Homo Cyberneticus comprises five volumes. All are relatively self-contained, but complement each other into one whole. The *first* volume is a synopsis of the simulation intentions and approach adopted by us; its tacit main question is "How can we operationalize these intangibilities?". The *second* volume tackles this problem on the methodological level by trying to elaborate a systematic epistemology for dynamic causal modelling. Its conclusions support our initial 'hunch' that simulation is the optimal tool for dymanic causal modelling. Volume *Three* is therefore an investigation into our possibilities of constructing a simulation tool (a computer program) which will allow us to concentrate on our problem instead of on the computer. We do not wish to adapt our problem to the computer, but expect the computer to adapt to our problem instead, so to speak. Volume *Four* leaves all computer problems behind and asks again how we can simulate human behavior; the investigaiton is this time less superficial than in the explorative first volume. The *fifth* volume, finally, delineates a framework for rendering the causal simulation of societal macro-processes possible. It is simultaneously a first example of how the principles discussed in the earlier volumes can constrain, generate, and guide large-model concretizations.

The tackled task is vast, and although everyone agrees that it needs be tackled, there seems to be a general suspicion that those who do it are charlatans and/or self-appointed prophets. It has hitherto been difficult to argue for *Homo Cyberneticus* properly because its 'point' lies in the linking together of a plurality of diverse fields of research, and also because it contains relatively much 'New Think'. Especially the advocation of common-sense thinking at the expense of statistics has evoked skepsis and territoriality defenses. Sociologists have thus downgraded it all to 'mere' computer science, while computer scientists have considered the simulation project oddly humanistic and too problem-oriented.

I was therefore very grateful when the Swedish Riksbank Tercentenary Fund dared to give financial support to my research in 1968. It has done so until this day.

The other important event was the foundation of the Computer Science Laboratory in Uppsala by Dr. Erik Sandewall and myself, as a 'home' for our up to then 'homeless' research projects, his being in the field of artificial intelligence, mine in sociocybernetics. This symbiosis has, at least for my part, been immensely stimulating.

I thank also my collegues *Lennart Stålberg, Torgny Tholerus, Tore Risch,* and *René Reboh,* most sincerely for their participation and cooperation in this sociocybernetics venture during the past years.

Uppsala, in April 1974 H.J.H.

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Part One

Reality and Representation

Science is very much concerned with communication by written or spoken language. It is fair to assume that all that can be said also can be written, if we only stipulate unambiguous correspondence rules (and vice versa). But written communication tends to have greater significance in science, and we shall therefore concentrate on it. We may choose to write for several purposes: (1) to keep track of our own thinking, in particular (1a) to save ideas which we perhaps otherwise will lose, and (1b) to maintain an overview; (2) to communicate with collegues and other interested persons, or (3) to communicate with computers. It is in all cases desirable that we write unambiguously; in case 2 it will be extra favorable if we also write 'elegantly' (in order to be read at all).

Empirical science is concerned with reality, and it is therefore necessary for empirical scientists to be able to think, speak, and write about reality. We shall in Part I of this booklet discuss generalities about reality, the apperception of reality, and symbolic representations of apperceptions and of reality conceptions in general.

1. Reality and Representation

1.1. Existence and Essence

Bocheński (1965, according to Adam 1970, p 18–19) tells us that the world consists of *things* that have *properties* and that are *related* to one another in various ways. *Reality* ('empiry') is all that *is* or that *can be*: things, properties, or relationships. Each part of reality is characterized by whether it *exists actually* or only *potentially*, and by its state ('*essence*'; cf. Habermas 1958, p 18, on 'Wesen').

We do not quite agree with Bocheński's account. To include mere possibilities among the things that constitute reality is against the common-sense meaning of "reality", and it seems unwise to stipulate a deviant use. Sartre's (according to Apel/Ludz 1958, p 90) suggestion that reality is all that one can become conscious of, is no better in this respect. Jaspers' (1966, p 73) explication that reality is what we meet in everyday life as substance or as resistance or constraints, seems closer to being adequate.

Anyway, the necessity to postulate a characteristic 'actuality of existence' stems from the inclusion of possibilities among what is real. Because then we must be able to distinguish what actually is real from what *could* be real now. (We thus wind up with things that are really real and things that are unreally real, and that seems to us quite dissatisfactory.) We stipulate therefore that "to be real" is synonymous with "to be actually real" and that only things can be real. Predications (properties or relationships) cannot be real, they are only more or less 'valid' claims. What it is that makes them 'valid' is equally difficult to tell as what is the difference between existence and non-existence, although it *seems* to us utterly obvious (cf. section 1.3.).

1.1.1. Reality and Apperception

Reality exists even if no one perceives and apperceives it. (This view is called 'ontological realism'.) Reality outside the perceiver is not changed by the act of perception. We can claim this despite Heisenberg's uncertainty principle: manipulations which are to enable or facilitate perceptions are not perception itself. The most essential concern of methodologies in all branches of science are, in fact, specialized rules and instruments for manipulating reality for the sake of enabling or facilitating perceptions.

1.1.2. Perceptions and Apperceptions

We shall use the word "apperception" to mean perceptions, processed in the central nervous system ('CNS') of a human being or an animal, that somehow change the state of that CNS.

A blind person can, according to this use of words, perceive but not apperceive if, for instance, the neural connections between his (otherwise healthy) eyes and the CNS are severed.

If I apperceive something red, I *perceive* electromagnetic waves. If I *apperceive* hunger, I *perceive* certain chemical states and/or physical processes (such as what I can apperceive as stomach contractions, for instance).

(Cf. Marc-Wogau 1945, which is a monograph on perception and apperception. See also Marc-Wogau 1967, p 123–219; Culbertson 1963; Klir/Valach 1967, chapters 14 and 16; Frank 1971, p 172 ff.)

Observe that we follow a quite common use of letting the same word denotate sometimes the *process* of perception ('perceiving') or apperception ('apperceiving') and sometimes the *result* of these processes ('percepts' or 'appercepts', respectively). The context will determine the intended meaning.

(Cf. Kirschenmann 1970, p 44–46, p 108, and p 156–171, on the problem of perception and apperception from a dialectical materialist's point of view, and on the terminology used in diamat.)

1.2. The Result of Apperception

The result of apperception is a representation of the apperceived empirical object or situation ('Sachverhalt') in the mind of the apperceiving subject. (We shall, in this text, not discuss the very intricate but significant problem of whether robots ever can apperceive all by themselves; but if this were the case, the resulting representation would be in the robot's mind-counterpart. Cf. Culbertson 1963.)

1.3. The Correctness of Apperception

It is forever impossible to prove that an apperception is 'correct', and it is even difficult to say what we mean by "this apperception is correct". It is therefore practical not to bother with questions concerning the properties of reality 'as such' (f.i., what 'red' *really* means) but to assume instead that we *can* apperceive correctly under certain 'normal' circumstances. For in-

stance, assume that A and B look at a willow tree at dusk. A believes to see a tree, B believes to see a man. *We* judge that A apperceived correctly, B incorrectly. But how can *we* know? Well, we can justify our judgment, for instance, by saying that (1) we *know* that A is right because we have seen that very tree in daylight (a more 'normal' condition), or (2) we have touched the thing and found it to be a tree (and we know of no circumstances where a man feels like a tree), or (3) B is near-sighted or intoxicated and therefore not 'normal', while A is 'normal'.

The important thing is that we can represent, and thus also communicate about, parts and features of reality, f.i., colors or temperatures, or somebody's absence, and that there is some agreement regarding the circumstances under which the referred-to phenomena are to be apperceivable. If A says "B is not here" and B is merely standing behind A, then we must say that if A knew where B is, he would agree that B in fact *is* there. A's saying "I don't see B" is not generally equivalent with "B is not here", because if A says this to C, and C sees B, then C can judge that if A were in C's position, or in any of a multitude of other positions (although still here), he would not claim that B is not here. That is: A would not 'normally' claim that B is absent.

1.4. Empirical vs. 'Aprioric' Insights

A person's first insights ('Erkenntnis') 'emerge' from perceptions of reality, but not all later insights are directly based upon perceptions or apperceptions of reality (Stache 1958, p 58). Some are 'analytic conclusions' from apperceptions. In fact, Kant has shown not altogether unconvincingly (in his famous 'Kritik der reinen Vernunft') that even apperceptions are based upon 'aprioric' insights (namely the 'categories' of apperception, such as the (according to Kant) a priori given conceptions of time and space); percepts are merely the 'triggers' for the generation of certain associated appercepts.

The assumption of 'aprioric' insights into the 'nature' of reality can lead to claims that "truth" and "falsity" (with regard to statements) denote the fact that a statement is self-contradictory or not; that is: *if* we knew everything about reality, we would realize how the 'nature' of things implies all their correct predications. (Kant seems to have held such a view which, in our opinion, is objectionable.)

What do we mean by saying "this statement is self-contradictory"? Marc-Wogau (1967, p 102, in an analysis of what Kant may have meant) suggests that it could mean "the predicate contradicts the concept of the

subject" (in subject-predicate sentences). This suggestion seems to us to be a good starting point for a further discussion of the meanings of concept compositions or even statement compositions. (This is not the right context to conduct such a discussion in, we shall therefore only make a few indicative remarks. Cf. Bourne 1966 and Gregg/Simon 1971, which contain relevant studies.) "It is sort of a square circle" would by most philosphers be regarded as a typically self-contradictory description, and likewise "I took a picture of a house without roof or walls". They would say that a circle *cannot* be square and a house *must* have walls and a roof. Well, it depends upon how you look at the meaning of words. If they are regarded as fixed pictures of reality (as Kant seems to have done), you will call the above statements self-contradictory. If they are viewed as labelling conventions, you can say that they have been used in an unorthodox manner, perhaps by a verbally untrained person, a child may be, and you interpret them therefore as a description of a super ellipse or of a ruin. (This use seems to be analogous to the use of counterfactual statements such as "If I were you, I would stay".)

This impossibility to reject what according to the first view are self-contradictions, does not diminish our alleged possibilities to obtain 'aprioric' insights.

1.5. The Question of Representation

It is as yet not known how facts are represented in the mind, and we shall not speculate on this problem here. We shall instead take hold of the fact that these representations in the mind can be translated into representations on paper. In order to achieve such a translation we need a sort of code, a set of *correspondence rules*, which stipulate correspondences between 'internal' representations (in the mind, that is) and 'external' ones (on paper or the black board, for instance). We say then that a certain 'external' representation *means* a certain 'internal' one.

'External' representations are called *signs*; more complex representations are also called *patterns*.

1.5.1. The Adequacy of Representation

Much of what is called science is, in fact, concerned with the representation of conceptions. Representations are not all equally practical, and the solution to a problem lies often in an 'adequate' representation. Whether a representation happens to be 'adequate' can unfortunately only be said a posteriori, namely when it, in fact, has enabled, facilitated, or stimulated

the production of a solution. Mathematics and technology would probably not have flourished as they do today if the Latin number representation had not been abandoned. It is, however, very easy to forget that an 'adequate' representation for given conceptions often was not at all *obvious* before it was found. That it seems a posteriori obvious is a sign (symptom) of its be ing relatively elegant (or, at least, not too awkward) in comparison with natural language.

McCarthy/Hayes (1969, p 469–476, with examples) have looked at representation adequacy from the artificial-intelligence point of view, and distinguish three kinds:

"A representation is metaphysically adequate if the world could have that form without contradicting the facts of the aspect of reality that in terests us.

. . . .

A representation is called epistemologically adequate for a person or machine if it can be used practically to express the facts that one actually has about the aspects of the world.

. . . .

A representation is called heuristically adequate if the reasoning processes actually gone through in solving a problem are expressible in . . ." it.

1.6. The Meaning of "Meaning"

Every representation has a 'meaning' if we know a correspondence rule which associates the representation with some other representation; that other representation is the meaning of the first one.

If this is to be true, what then is the meaning of an 'internal' represen tation? Well, trivially, of course, it is any of the 'external' representations that we have associated with it. But that is not the point of the question, since we can have 'internal' representations without having 'external' ones, and we must still be able to say that the 'internal' representations have meaning. This touches again upon the apperception problem. We slowly but steadily, and especially during infancy, build up correspondence rules between certain internal (neuro-physiological) states and empirical givens. Reality is the ultimate meaning.

Many philosophers and religious people have continued to ask "What is the meaning of reality?" but we believe that reality is not a representation or expression of something else, of the intentions of some God, for in stance. Reality is simply reality.

(Cf. Ogden/Richards 1952; Kirschenmann 1970, p 38, p 172–178;

Schaff 1962, p 212ff, with a detailed bibliography (according to Kirschenmann 1970, p 198, note 113); Stiehl 1970.)

1.7. Signs

A *sign* (or *token*) is a physical phenomenon, that is, a material object, or a non-empty sequence of states of a material system. But not all physical phenomena are signs; they are signs only as long as they are *interpreted* as such.

A sign can be interpreted as a sign *for* something; in that case it is often called *symbol*. A sign can also be interpreted as a sign *of* something; in that case it is often called *symptom*. If we stipulate that symbols always are signs that have obtained meanings by social agreement (conventions), while symptoms never obtain their meaning by convention, then it is easy to maintain a clear-cut distinction.

The interpretation of a physical phenomenon as a sign is, of course, first of all an apperception (a *recognition*) of the physical phenomenon itself, but also of something 'beyond' it. That which is 'beyond' the sign is the sign's meaning (in the case of symptoms we often say "implication" instead of "meaning").

(Cf. Morris 1960; Kirschenmann 1970, p 24–31, p 43–46, p 172–178; Meyer-Eppler 1969, p 309ff; Flechtner 1968, p 52ff; Frank 1971, p 39–52; Stachowiak 1973, p 196ff.)

1.7.1. The Use of Signs For Communication

This text disregards the functioning of signs as symptoms because we believe that spoken or written scientific communication ought to be as explicit as possible. It ought therefore be performed with the help of signs as symbols. Symptoms are to occur in science only as the result of apperceptions of empirical reality.

1.7.2. Communication

A falling apple 'communicates' with a flower as it hits and crushes it; in a wide sense is all influence also communication. In this text, at least, we shall not use "communication" in this wide sense, but always require that it involves the use of symbols. In particular, two computer modules communicate iff certain designer-intended signals go from one to the other; any unintended vibrations, or heat radiations, that go from one to the other, are not communication. (The designer intention is in this case equivalent to a convention common to the designer and the two modules. We must

assume that the two modules cannot agree upon conventions all by themselves.)

(Cf. Flechtner 1968; Mayer-Eppler 1969; Kirschenmann 1970, p 20–23, p 31–32; Reimann 1968; Neuburger 1970; Beckenbach/Tompkins 1971.)

1.8. Language

Written or spoken symbols are called *words*. Strings of words are called *sentential forms* if their composition has followed certain conventional rules ('syntactical rules'). A sentential form is called *sentence* if it has a meaning; this meaning is said to be the *statement expressed by* the sentence. A set of syntactical rules is called *syntax*. A set of rules which define correspondences between conceptions and sentential forms is called *semantix*. The (usually potentially infinite) set of all sentences that can be generated in accordance with a given syntax and a given semantix is called *language*. (Cf. Pirotte 1970; Klir/Valach 1967, chapter 15; Gross/Lentin 1967.)

1.8.1. Statements

Statements can be descriptive, prescriptive, interrogative, enumerative, performative (J.L. Austin according to Wright, 1965, p 218),or declarative.

1.8.2. Descriptive Statements

Descriptive statements are characterized by the fact that they are the only statements that can be assessed to be true, false, possibly true, probably false, sometimes true, and so on.

Questions, wonderings, commands, implorations, promises, wishes, permissions, or indications, cannot be true or false. It is usually assumed, that evaluative statements are non-descriptive. The truth of the statement expressed by "She is truly beautiful" can, according to this view, not be assessed. (See: Hägerström 1939; Russell 1937, chapter 9.)

It seems to us that this is counter-intuitive (similarly: G.E. Moore, M. Scheler). We are instead more inclined to assume that expressions such as "x is good" express incomplete statements and can therefore not be truth-evaluated in isolation. After all, "It is raining" can similarly only be evaluated if we make assumptions about the where and when of the referred-to state of the world. "x is good" requires, for its evaluation, a definition of "good" which supplies recognition criteria, as well as a description of x in terms relevant for the recognition of goodness in x.

However, we accept subjective recognition criteria (as have been proposed, for instance, by Westermarck (1932), or, according to Hedenius (1965, p 127), by Hegel) only for subjective value judgments. "This music is perfect" may thus be interpreted as expressing a subjective value judgment; if you feel that way, no one can say that you are wrong. But "This music is perfect" may instead be meant to state that a certain piece of music seems instrumentally adequate in the score of a tv spot for evoking a certain mood. The latter statement can be tested empirically.

Any generalization of subjective value judgments such that x is (intrinsically) good if the majority of all people (all presently living? or all that have hitherto lived? or all that will ever live and ever have lived?) would agree, can only be a convention, never a fact. Conventions cannot be true or false, but more or less practical or established. A generalized subjective value judgment can therefore not be said to be true or false, although it is often deceptively similar in form to a descriptive statement. For instance, does "Arabic horses are elegant" express an evaluative or a descriptive statement? Is it synonymous with the empirically testable "Arabic horses are considered elegant"?

The distinction between evaluative and descriptive statements is evidently difficult and it may therefore seem impractical to treat them as essentially different. After all, the statements expressed by "All men are mortal" or "Grass is green" are also generalizations from individual observations. But we do not wish to conclude that these generalizations are merely conventions. One wants them to be induced facts or rules, and tends to claim that being mortal or being green are 'objective' characteristics, while being elegant is subjective. But would Arabic horses suddenly cease to be elegant if mankind disappeared and Arabic horses therefore no longer could be 'described' as elegant? The claim that grass is green could likewise not be made if mankind had disappeared—is it therefore a generalized value judgment?

It is possible to look upon evaluative and descriptive statements as opposite and fuzzy end regions of a one-dimensional continuum. This continuum is constituted by the values of the intersubjective variability of judgments or descriptions. That grass is green is generally accepted and therefore said to be an 'objective fact', while it is *known* to be dependent upon personality characteristics whether one judges a certain other person as charming or not. Observe that we need not refer to majorities or total populations in order to be able to assign approximative scale values. But our view implies that it is a matter of knowledge regarding variabilities, and thus *indirectly* conventional, whether or not we let a statement be ascribed truth or falsity; besides, where we set the dividing point on the scale can be

made a *direct*, explicit convention after one has agreed upon a variability measure that maps onto this continuum.

This view has significance for our look upon 'social facts'. Extrinsic values can be assessed relatively easily and are therefore much less variable than intrinsic values. Predictions with extrinsic values are hence easily acceptable as claiming provable facts. Other predictions, on the other hand, cannot be a priori distinguished from value judgments and remain thus forever at least slightly dubious as claims of fact.

1.8.3. Descriptions

A *description* is a composition of sentences which express *descriptive* statements.

1.8.4. Conceptions

A *conception* is a composition of descriptive statements.

1.8.5. Propositions

We shall now introduce the word "proposition" into our vocabulary and stipulate that "the proposition p" be synonymous with "the descriptive statement expressed by the sentence p" or with "the descriptive statement p", depending on the form of p. For instance, if we say "He made the proposition p", it seems sufficiently obvious that the second-mentioned meaning is most adequate, while "He gave me a list of the propositions he had made" rather unambiguously requires the first-mentioned interpretation.

1.8.6. Semiotics

Carnap (1942, p 8–10 (according to Martin 1959, p 2)) distinguishes three fields (together called *semiotics*) of investigation of languages:
pragmatics comprises studies of the relationship between a language user and the language used;
semantics abstracts from the user and comprises only studies of relationships between expressions and their designata;
syntax (we would prefer *syntactics*) abstracts from designata and comprises only studies of relationships between expressions.
(Similarly: Morris 1960, Kirschenmann 1970, p 25–26; Flechtner 1968, p 54; Frank 1971, p 39–52.)

1.9. Formal Languages

In scientific communication one nowadays frequently uses formal languages. Formal languages are *invented* languages and differ from natural ones in having a much simpler *syntax*. The translation of a natural-language sentence into a formal-language sentence is called *formalization*.

Language design and formalizations are essential parts of computerized problem-solving. But the use of invented languages is not restricted to science alone: chess notation and the notations used for knitting instructions are examples of very simple formal languages, designed for the purpose of rendering messages more concise.

Although it is always possible to translate a formal language sentence into a natural-language sentence, the result is often extremely clumsy. (For instance, translate a matrix equation into natural language.) A formal language can, if one wishes to do so, be regarded as an extension of a natural language, which one arrives at by explicitly specifying substitution rules.

It is possible to invent formal languages for any specific purpose, but that does not mean that we can concisely express whatsoever after we have been given a set of formal languages. In order to be easy-to-learn, easy-to-communicate, and unambiguous, formal languages are usually kept simple and purpose-specific. Natural languages, in contrast, are ambiguous, general-purpose languages with difficult grammar; to describe specific phenomena in natural language means that one presupposes no familiarity at all with the described phenomena on the side of the listener or reader; one may therefore not use words or terms which have a meaning of not generally known definition (f.i., 'white dwarf', 'Herzsprung-Russel diagram'). But if the speaker/writer and the listener/reader remain in communicative contact for a while, and the regarded aspect of reality also remains the same, we will find that the speaker/writer increasingly *presupposes* agreements about word meanings, for ease and elegance of communication. A *content of discourse* is established. To enter as a newcomer into an established context of discourse is usually frustrating, be it a cocktail party where you join a group that discusses the artistic intentions of someone called *he*, or to start reading this booklet from the middle.

1.9.1. Ancillary Statements

Scientific texts are usually conglomerates of descriptions, prescriptions, interogations, and evaluations. Of all the descriptions, only one concerns the text's focal subject matter. All other expressions (sentences) either concern various aspects of the very research process, or other descriptions in turn.

There can be accounts of the history of certain concepts or conceptions, there can be specifications of data-collection procedures used, there can be criticisms of methods or theories, or replies to criticisms, and there can be language-use stipulations. All these other ('ancillary') statements serve the purpose of guiding the reader's text interpretation.

We can, of course, choose different languages for the different descrip tions, for the sake of clarity. It is not unusual that the focal description is formalized, while the commenting descriptions or prescriptions are for mulated in a natural language.

1.9.2. In Defense of Specialized Language

It is always of uttermost importance to write comprehensible, of course, and thus also to make ample use of ancillary statements. But it is not *always* necessary or desirable that one writes 'for the people'. If one were never to lift from the firm common base of natural language, it would soon be incredibly cumbersome to communicate with interested others. A writer must be allowed to presuppose that those who *wish* to read what he has written also temporarily accept the language conventions that underlie his text. The writer, in turn, has the obligation to inform about these conventions.

Each specific-language publication ought therefore include references to the texts which elaborate the conventions one has adopted, insofar as the author has chosen not to review these conventions once more explicitly in his own text.

Part Two

Epistemological Categories

In Part Two we shall first introduce a somewhat formalized language which we then immediate shall use for facilitating the definitions of a great number of otherwise very 'fuzzy' concepts which we daily encounter in discussions among social scientists (and life scientists in general), such as system, behavior, structure, relationship, function, mechanization, feedback, organization, complexity, or model.

As yet, there has not been an established specific language for the description of life phenomena (that is: biological, psychological, or social phenomena), a fact which has hampered the development of especially the social sciences, since they have least in common with the natural sciences, and therefore can make relatively little use of the representation conventions used there.

The specialization of the language presented here reflects a particular world view, namely that of general cybernetics, which we believe to be able of giving the whole life-science field a common structure. Such a common structure is of multiple use: it is epistemologically economical, it facilitates orientation and communication, and it enables knowledge accumulation and logical consistency tests.

Please note that a description of a life phenomenon in terms of some specialized language, does not, by itself, achieve an explanation, and it must not be mistaken as such. It can, at best, be a first and necessary step towards an explanation. In particular: we do not consider it to be an explanation if we merely redescribe a phenomenon in systems-theoretical terms, unless the redescription has enabled us to discover relationships between various aspects of the phenomenon that were obscured before.

We beg mathematically and logistically untrained reader not to despair at the sight of formulae, but merely to read slowly and systematically, and to learn the formula-writing and -reading conventions as we present them. But skip complicated details and return to them if and when they are needed for understanding a later passage of the text.

For those who feel that they need a less concise introduction into set theory and general cybernetics, we wish to recommend a few books which we have found to be *very* easy to read and from which we ourselves obtained our first stimuli once upon a time:

Ashby 1956, An Introduction to Cybernetics.

Klir/Valach 1967, Cybernetic Modelling.

Kemeny/Snell/Thompson 1966, Introduction to Finite Mathematics, 2nd ed.

Flechtner 1968, Grundbegriffe der Kybernetik.

Anschütz 1967, Kybernetik—kurz und bündig.

Klaus 1968 is a dictionary of cybernetics, Steinbuch 1971[4] and Bertalanffy 1968 discuss general principles of cybernetic thinking and Lohberg/Lutz 1968 contains a collection of amusing applications of cybernetic conceptions to everyday life.

Quite recently appeared a book by Fararo (1973) on mathematical sociology which also seems recommendable.

2. Sets and Relations

2.1. What are Data?

Usually, when we think of data 'naively', we imagine them as being bits of a picture or description of reality, very much like parts of a jigsaw puzzle. We believe that in order to learn which relationships exist between obtained pieces of data we have to use different forms of data analysis; this undertaking does not guarantee success, but we normally do not see any other possibilities.

(2.1a) The pieces of data are called *variables*, each variable is assumed to be *able to vary* (that is why variables are called variables). What does this mean?

(a) In the case of non-empirical data it means simply that the variable is a category of different values, of which one by some particular rule is determined to be the variable's *'actual' value* for the time being and in the context under study;

(b) in the case of empirical data it means that we assume that the information which we can expect to obtain when observing a particular part of empiry, can vary for any number of known and/or unknown reasons.

We can also in this latter case abstract to a category of values being (at least associated with) a variable.

(2.1b) Set theory is a simple theory about such categories of values and about relationships between them. In set theory one thus says "*set*" instead of "variable", "set element" instead of "variable's value", instead of "variable X has the value w" one says "*w is element in X*". The attribute of being element in a set is a relationship between a set and an element; it is therefore called *attribute relation* or *element relation*.

Relationships between variables, which we try to 'reconstruct' with the help of data analysis, are called *structural relationships* and are really relationships between attribute relations.

Relationships which are not attribute relations are structural relations. Also: If we assume the attribute relation to exist between any X and any w, then we may not simultaneously and in the same context assume the existence of a structural relation between X and w. However, several structural relations may coexist between X and w if w is not element in X.

(2.1c) The set-theoretical answer to "what are data" is thus: *data are sets of attribute and structural relations.* (Mealy 1967.)

2.2. Sets and Subsets

(2.2.1) *Explication*: A *set* is a well-defined aggregate of mutually distinguishable objects. These objects are called *set elements* or *set members*.

(2.2.2) *Remark*: Anything can be a set element, concrete phenomena as well as abstract ones, real ones as well as hypothetical ones, dynamic ones as well as static ones, and complex and/or complicated ones as well as simple ones.

(2.2.3) There are two essentially different ways of *defining a set*. Defining a set M amounts to delimiting M within another set W, which contains *at least* all members of M. This can be done either

(a) by *listing* all the members of M, or

(b) by *listing* a number of criteria which together suffice to decide for each element of W whether it belongs to M or not.

In the first case we are said to specify the *extension* of the set M, or to *define M extensionally*. In the second case we are said to specify the *intension* of the set M or to *define M intensionally*. When one defines a set extensionally, that is with the help of a list, this list is usually delimited by the signs "{" and "}".

Examples: (a) Extensional definition of a set W:

$W = \{1,2,3,5,8,13,21,34\}$

(b) Intensional definition of a set M:

M contains all elements of W which are not multiples of 2.

(c) Extensional definition of this same set M:

$M = \{1,3,5,13,21\}$

(d) Intensional definition of a set P:

P contains all elements of W which do not belong to M

(e) Extensional definition of P:

$P = \{2,8,34\}$

(2.2.3a) The *cardinality* (or *volume*) of a set is the number of elements contained in its extension.

(2.2.4) A set M which has all its elements in common with another set W, is said to be a *subset* of W. M is said to be a *proper* subset of W if W contains at least one element not contained in M and if M contains at least one element.

Examples: Let $W = \{1,2,3,5,8,13,21,34\}$ and $M = \{1,3,5,13,21\}$. M is found to be a *subset* of W, since all elements of M are also contained in W. M is furthermore a *proper* subset, since the elements 2, 8 and 34 of W are not contained in M, and M does contain at least one element.

Let $P = \{1,2,3,5,8,34\}$, and $Q = \{1,2,3,5\}$. We find that P and Q are proper subsets of W. Q is also a proper subset of M and also of P. But P is

not a proper subset of M, since 8 and 34 are not contained in M. Nor is M a proper subset of P, since 13 and 21 are not contained in P.

(2.2.4.1) As a logically convenient theoretical construct we introduce the concept *empty set*, denoted and extensively defined by the empty list "{ }". We postulate that the empty set is *unique* since two empty sets are indistinguishable by their extensive definitions.

The empty set is an improper subset of any set.

(2.2.4.2) According to (2.2.4) is every set an improper subset of itself.

(2.2.5) As we have seen is it not necessary to have a specific mathematical notation in order to develop a mathematical theory. Such a notation is nothing but a repertoire of abbreviations for descriptions of frequently encountered relationships. As such it is convenient, but of no own theoretical importance. It may admittedly slow down one's reading speed initially if one has to decipher new symbols, but eventually it is significantly increased when it comes to reading expressions.

Example: "Add 1 plus 2 plus 3 plus 4 plus 5 and divide this by 3" is more conveniently written "(1+2+3+4+5)/3".

Let us adopt the following established abbreviations:
"a εA" *for* "a is a member of a set A"
"a $\notin$A" *for* "a is not a member of A"
"M$\subseteq$W" *for* "M is a subset of set W"
"M = W" *for* "M and W have the same extensions"
"M = $\varnothing$" *for* "M is the empty set", this may also be written "M = {}"
"M $\neq \varnothing$" *for* "M has at least one member"
"M$\subset$W" *for* "M is a subset of W, and W contains at least one member not contained in M".

2.3. Operations on Sets

(2.3.1) In mathematics one uses very often the same word and the same symbols in order to denote an *operation* and the *result* of this operation. For instance, "3+5" means the instruction "add 3 and 5" and it also means "8".

Operations on sets result in new sets. We shall introduce four such operations, which are denoted by $\cup$, $\cap$, $\setminus$, and $\times$.

"W$\cup$M" shall mean "the set which contains all elements of W plus all elements of M not contained in W". Alternatively it means the operation which produces such a set. W$\cup$M is said to be the *union* of W and M.

Example: Let M = {a,b,c} and W = {a,d,g}. The union of M and W is then {a,b,c,d,g}.

"W∩M" shall mean "the set which contains all elements of W which also are members of M". Alternatively, it means the operation which produces such a set. W∩M is called the *intersection* of W and M.

Example: Let M = {a,b,c} and W = {a,d,g}. The intersection of M and W is then {a}.

Let P = {a} and Q = {b,g}. The intersection of P and Q is then {}.

"W\M" shall mean "the set which contains all elements of W *except* those which are contained in M", alternatively the operation producing this set. W\M is called the *complement* of M in W.

Example: Let M = {a,b,c} and W = {a,d,g}. The complement of M in W is easily obtained by removing those elements which W has in common with M, it is here = {d,g}.

Let P = {a} and Q = {b,c}. Then P\Q = {a}.

(2.3.1a) But observe that {{a,b,c}}∩{a,b,c} = {}, while {{a,b,c}}∪{a,b,c} = {{a,b,c},a,b,c}. That means that sets can be elements in other sets: {a,b,c}ε{{a,b,c}, a,b,c}.

But it is *incorrect* to write {a,b,c}ε{a,b,c}, or a⊂{a,b,c}, or {a,b,c} = {{a,b,c}}.

(2.3.2) The set denoted by "M×W" is called the *Cartesian product of M and W*, or also the *universal relation between M and W*. M×W is the set which is the result of forming all ordered pairs which can be formed with the elements of M and W such that each ordered pair's first element is picked from M and the second from W.

Example:

Let R = {a, b, c, d, e, f, g, h} and

C = {1, 2, 3, 4, 5, 6, 7, 8}. R × C contains then the 64 elements (a,1), (a,2), (a,3), . . . , (h,7), (h,8).

The elements of R×C are called *ordered pairs. They are not sets.* The following is therefore valid:

(a,1) ≠ {a,1}

(a,1) ≠ {(a,1)}

(a,1) ε {(a,1)}

(a,1) ∉ {a,1}

(a,1) ≠ (1,a).

(2.3.3) Universal relations are also often called *co-ordinate systems, property spaces, state spaces,* or *classifications*. (Cf. Asplund 1968.)

(2.3.3a) Cross-tabulations and chess games are examples of processes that define structures upon Cartesian products.

2.4. Relations

The 'naive' conception of a relation between a number of objects implies presumably that one imagines the existence of removable 'links' between these objects. These 'links' are in that case mental constructs which express coordinations of objects. Each of these 'links' can be associated with a tuple. A relation is then seen to be describable as a set of tuples. It is always defined on given sets of objects. For instance, the relation TWICE *structures* the set {1, 2, 3, 4, 5, 6} by coordinating certain elements into tuples; these tuples are (2,1), (4,2), and (6,3). The set {(2,1), (4,2), (6,3)} is called the *extension* of TWICE in {1, 2, 3, 4, 5, 6} × {1, 2, 3, 4, 5, 6}. To give a further example: The relation LESS-THAN corresponds to the ordered pairs (1,2), (1,3), (1,4), (1,5), (1,6), (2,3), (2,4), (2,5), (2,6), (3,4), (3,5), (3,6), (4,5), (4,6), (5,6).

(2.4.1) These two relations, TWICE and LESS-THAN, are evidently associated with proper subsets of {1,2,3,4,5,6} × {1,2,3,4,5,6} as extensions.

This can be generalized and formulated as a postulate:

A *relation* between n variables V_1, V_2, ..., V_n is a subset R of the Cartesian product set, $\times V_i$, in symbols: $R \subset \times V_i$. ($V_1, V_2, \ldots, V_n$ are called *the arguments of R*.)

Relations between 2 variables are often called *correspondences*.

Remarks: In case $R = \times V_i$, R is called the *universal relation*. It is obvious that (a) every non-universal relation is a proper subset of the universal relation, and that (b) relations can be ordered according to restrictiveness (that is, according to their cardinalities; the ordering involves ties and is therefore called *not strict*). The universal relation is always the unrestrictive relation between variables. We have also seen above that TWICE there was more restrictive than LESS-THAN.

Relations which are neither universal nor empty are called *proper*.

(2.4.1.1) We say that a relation R is a *structure on* a set $\times V_i$ if R is more restrictive than the universal relation. $\times V_i$, on the other hand, is then said to be *structured by* R.

(2.4.1.2) *Remark*: In mathematics one tends to replace, whenever possible, intensional by extensional definitions. It is namely much easier to manipulate with, and to compute on, extensions than with intensions. Intensional definitions, in contrast, are often more concise than extensional ones; it is presumably for that reason why intensional definitions are more usual in non-mathematical thinking. A compromise is necessary when it is practically or theoretically impossible to write out the complete list of elements of a relation extension. The relation LARGER-THAN, f.i.,

defined on N×N, if N is the set of all integers, has an extension of infinite cardinality; it can be symbolically specified by "LARGER-THAN = $\{(n_i, n_j) | n_i > n_j\} \subset N \times N$", which is read as "the relation LARGER-THAN contains all and only those ordered pairs $(n_i, n_j) \varepsilon N \times N$ *which fulfil the condition* that n_i is larger than n_j". ("which fulfil the condition" or "such that" is here, as is usual, expressed by the stroke sign" | ".)

(2.4.2) A relation with n arguments is called *n-adic relation.* The frequently occurring 1-adic, 2-adic, and 3-adic relations are also called *monadic, dyadic,* and *triadic* relations, respectively. Monadic relations are rather infrequent; existence and command operators (as in "God exists", or "stop it!") are examples of monadic relations. (Dyadic relations are sometimes also called *binary*, but this terminology is not recommendable since it has become ambiguous.) Proper relations which are not monadic nor dyadic (which thus are n-adic, with n>2) are called *polyadic.* A particular class of relations describe relationships that can hold between a variable number of objects; such n-adic relations with varying n are called *variadic.* The Cartesian product is an example of a variadic relation with at least two arguments; in *concrete* instances, however, it is always either dyadic or polyadic.

Dyadic relations are of prominent importance because of their frequency of use in descriptions; this, in turn, is due to their simplicity. Theoretically can all polyadic relations be transformed into structured sets of ordered pairs. All we need to assume is that "(a,b)" also can be written as "(a,(b,()))". (Don't worry if this latter expression seems incomprehensible to you: it shall, by definition, *mean* "(a,b)". We have here introduced a constant, "()", called *nil* or *empty list,* as a theoretical construct used for indicating the termination of the 'nesting'.) Given (a,b,c,d), f.i., we can then rewrite it as "(a,(b,(c,(d,()))))" or simply "(a (b (c (d, nil))))". (Cf. McCarthy 1967, p 460–461.)

If we now henceforth mainly speak of dyadic relations, it is therefore no restriction on the validity of the discussion for polyadic relations. In consequence, when we, for convenience, merely write "relation", we shall in this text mean "dyadic relation" unless another meaning is obvious.

(2.4.3) Assume a relation to be given which has $R = \{(a_i, b_j)\} \subset A \times B$ as its extension. We denote this relation by "R(A,B)". Those $a_i \varepsilon A$ which are *first* components in *at least one* of the ordered pairs in R are said to *belong to the domain of R (A,B).* Those $b_j \varepsilon B$ which are *second* components in at least one of the ordered pairs in R are said to *belong to the range of R (A,B).*

Example: $R = \{(1,3),(2,5),(4,9)\}$. The domain of R is the set {1,2,4}, the range of R is {3,5,9}.

We denote the domain of a relation, R, by "dom (R)", its range by "rge (R)". Thus: If R = {(1,3),(2,5),(4,9),(5,11)}, then dom (R) = {1,2,4,5} and rge (R) = {3,5,9,11}. The elements of dom are called *first coordinates*, the elements of rge *second coordinates*.

(2.4.4) If every element in the domain of a given relation R is coordinated to exactly one element in its range, the relation is said to be *single-valued* or *determinate*.

Examples: Let R = {(a,1), (b,2), (c,3)}. R is determinate since

a b c
↓ ↓ ↓
1 2 3

Let P = {(a,1), (b,1), (c,4)}. P is determinate although a and b are coordinated to the same element in rge (P); but it is in all cases *exactly one* cor respondent:

a b c
↓ ↓ ↓
1 1 4

Let Q = {(a,1), (a,2), (b,3), (c,4)}. Q is *indeterminate* or *many-valued* since a has two different correspondences in rge(Q):

a b c
↓ \ ↓ ↓
1 2 3 4

(2.4.5) Single-valued relations are also called *functions* or *mappings*.

2.5. Functions

The term "F(x,y)" is conventionally interpreted as a relation if one assumes simultaneity of x and y, or if one ignores time order altogether. However, descriptions of processes cannot disregard occurrence orders, and thus not time. If we thus assume that y occurs *later* than x, then we say that F(x,y) is a *function*, and we may prefer to write "y:=F(x)" or "F(x)=:y" instead, in order to make also a visible distinction between a relation and a function. (This explicit admission of the temporal or computational definition ordering among the arguments is in disagreement with, for instance, Schmidt (1966, p 118) who also cites Halmos. It must not be forgotten that pure math does not care about reality. But we do.)

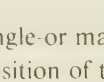
Functions are usually assumed to be *determinate* (single-valued) unless explicitly attributed otherwise. Relations, on the other hand, are usually

assumed to be many-valued, unless explicitly attributed otherwise. We adopt these conventions also in this text. (Cf. Schmidt 1966, p 129ff.)

Functions are thus to be understood as simply a specific type of relations.

(2.5.1) *Stochastic functions* are both determinate *and* indeterminate relations, depending upon the perspective: At any single point of time are they found to be determinate, but over time are they found to be many-valued. That is: The elements of the extension of such a function are replaced from time to time. This can be the consequence of an incomplete description of arguments. (When we write 'function' we do not mean to include stochastic functions.)

Example: Let R = {(a,1),x), ((a,2),y),((b,1),y),((b,2),z)}. R is determinate. But assume that we inadequately describe dom(R) as being {a,b} instead of {(a,1),(a,2),(b,1),(b,2)}. In that case will we find R to be indeterminate in a and b over time:



(2.5.2) A structure, S, on a set M × N is said to be *determinate* or *unambiguous* if and only if S is a function.

2.6. Properties of Relations

Besides single-or many-valuedness, relations can show other regularities in the composition of their extensions. We shall mention just a few classes of these.

Given be a relation R⊂A×A. Let a,b, and c be *arbitrary* elements of A.

(a1) R is called *reflexive* iff (a,a)εR.

(a2) R is called *irreflexive* iff (a,a)∉R.

(a3) R is called *non-reflexive* iff it is neither reflexive nor irreflexive.

Examples: SIMILAR is a reflexive relation; any object is namely similar to itself. NORTH OF is an irreflexive relation; no object can be north of itself. ABLE-TO-DESTROY is non-reflexive; an arbitrarily chosen object may be able to destroy itself (a bomb, f.i.) or not to destroy itself (a stone, f.i.).

(b1) R is called *symmetric* iff (a,b)εR implies that (b,a)εR.

(b2) R is called *asymmetric* iff (a,b)εR implies that (b,a)∉R.

(b3) R is called *antisymmetric* iff (a,b)∉R implies that (b,a)εR.

(b4) R is called *non-symmetric* iff R is neither symmetric, asymmetric, nor antisymmetric.

Examples: MARRIED-TO is a symmetric relation; if a is married to b,

then should b be married to a. PARENT-OF is an asymmetric relation: if a is a parent of b, then can b not be parent of a. LESS OR EQUAL is an antisymmetric relation: if a is not younger than, nor equally old as b then must b be younger than a. LIKES is a non symmetric relation: if a likes b, it is neither certain that b likes a, nor that b dislikes a.

(c1) R is called *transitive* iff $\{(a,b), (b,c)\} \subset R$ implies that $(a,c) \varepsilon R$.
(c2) R is called *intransitive* iff $\{(a,b), (b,c)\} \subset R$ implies that $(a,c) \notin R$.
(c3) R is called *non-transitive* iff R is neither transitive nor intransitive.

Examples: BROTHER-OF is a transitive relation: if a is b's brother, and b is c's brother, then is a also c's brother. FATHER-OF is an intransitive relation: if a is the father of b, and b is the father of c, then is a the grandfather, not the father of c. FRIEND-OF is a non-transitive relation: if a is a friend of b's, and b is c's friend, then it is still neither sure that a is a friend of c, nor that he is not.

2.6.1. Equivalence Classes and Abstraction

R is an *equivalence relation* if R is reflexive, symmetric, and transitive. The domain and the range of an equivalence relation are identical. If one structures a given set with an equivalence relation, the set is *partitioned* into a number of subsets, called *equivalence* classes. (A *partitioning* of a set is the distribution (according to some structuring rule) of all set elements into a number of mutually exclusive subsets. $\{\{1\},\{2,3\},\{4,5,6\}\}$ and $\{\{1\},\{2,3,4,5,6\}\}$ are, f.i., 2 different partitionings of the set $\{1,2,3,4,5,6\}$.)

If S is a non-empty set and Q is a partitioning of S into equivalence classes, we say Q also to be a *quotient set of S generated by R* and denote it by "S/R". The process of generation of S/R is called an *abstraction from S into S/R*.

Example: Given the set $S=\{a,1,b,2,c,3\}$. Let us define an equivalence relation called SIMILARITY such that $\{(1,2),(2,3),(a,b),(b,c)\} \subset$ SIMILARITY. Then must also $\{(1,1),(2,2),(3,3),(a,a),(b,b),(c,c)\} \subset$ SIMILARITY since it is to be reflexive. $\{(2,1), (3,2),(b,a),(c,b)\} \subset$ SIMILARITY since it is symmetric. And $\{(1,3),(3,1),(a,c),(c,a)\} \subset$ SIMILARITY since it is transitive.

S/SIMILARITY $=\{\{1,2,3\},\{a,b,c\}\}$ is a quotient set obtained by abstraction. This set contains only two elements: The class of all numbers found in S, and the class of all letters found in S.

2.6.2. Composition, Closure, and Connectedness

(2.6.2a) Let $P \subseteq A \times B$ and $Q \subseteq B \times C$. Assume a relation $R \subset A \times C$ which consists of all ordered pairs $(a,c) \varepsilon A \times C$ for which the condition is fulfilled that there exists some $b \varepsilon B$ such that $(a,b) \varepsilon P$ *and* $(b,c) \varepsilon Q$. R is then called

the *composition of P and Q* and denoted by "R = P o Q".

Example 1: Let P = {(1,a), (2,k), (3,f), (4,h)} and Q = {(g, alfa),(k,beta),(f,gamma),(a,delta)}. P o Q = {(1,delta),(2,beta),(3,gamma)}. Q o P = {}.

Example 2: Assume P and Q as recently, and further T = {(alfa,4),(beta,3),(epsilon,1)}. Q o T o P = {(g,h),(k,f)}. P o Q o T = {(2,3)}. T o P o Q= {(beta,gamma)}.

(2.6.2b) A relation R is said to be *closed,* or to have the closure property, iff rge (R)⊆dom (R). Closure is a significant property if a relation R with finite and non-empty extension is to be composed with itself to yield R o R. Iff R is closed, namely, is R o R guaranteed to be non-empty. "R o R", by the way, is also written "R^2", "R o R o R" is synonymous with "R^3" and also with "R o R^2", etc.

Example: Let R={(1,2),(2,3),(3,4)}. R (1) =:2,R^2(1)=:3, R^3(1) =:4, but R^4(1) is undefined. Instead, let P ={(1,2),(2,3),(3,4),(4,2)}. P(1) =:2, P^2 (1) =:3, P^3 (1) =:4, P^4 (1) =:P(1), P^5 (1) =:P^2(1), and P^n(1) =:P^{n-3}(1) for all n>3.

(2.6.2c) Assume the following function F, which we specify in a simplified notation:

	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w
F:	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	↓
	w	h	a	t	i	s	t	h	e	r	e	s	u	l	t	i	n	g	g	r	a	p	h

Let "a→w" or "w←a" or "↓" or "↑" mean that a single computation of F(a) yields w. Let similarly "a→w→h" mean that F(F(a))=: F(w) =:h. Let "h↻" mean that F(h) =:h.

The following figure is then a graphical illustration of F when computed repeatedly:

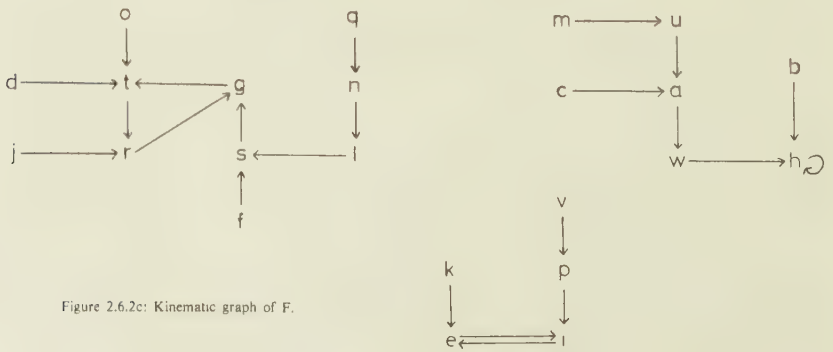


Figure 2.6.2c: Kinematic graph of F.

One can see clearly that the kinematic graph of F (as this type of il lustrations is called) is *disconnected.*

Let R be any relation, let a ε dom (R) and b ε rge (R). b is called *R-accessible from a* if (a,b)εR or if (a,c)εR and b is known to be R-accessible from c. We see then, for instance, that b is not F-accessible from a, but that h is F-accessible from both a and b. t is F-accessible from o and from g and also from t itself. k is not F-accessible at all.

A relation is called *connected* if every element in its range is accessible from every element in its domain.

Remark: The observation of disconnectedness in a function is of practical significance when it comes to predicting future states of development when the present state is known and if one from this knowledge can deduce which states are still accessible and which are not.

2.7. Fuzzy Sets

A *fuzzy set* is a class of objects with a continuum of grades of membership (Zadeh 1965, p 338). Let $X = \{x\}$ be a non-empty set. A fuzzy set Z is characterized by a membership function $f_Z(x)$ which associates with each element of X a real number in the interval (0,1); this number is the 'grade of membership' of x in Z (Zadeh 1965, p 339).

Such fuzzy sets play an important role in human thinking. "Essentially, such a framework provides a natural way of dealing with problems in which the source of imprecision is the absence of sharply defined criteria of class membership rather than the presence of random variables . . . In fact, the notion of a fuzzy set is completely nonstatistical in nature." (Zadeh 1965, p 339, p 340.)

2.7.1. Generalizations of Nonfuzzy Set-Theoretical Concepts

A fuzzy set is *empty* if its membership function is zero for all x in X.

Two fuzzy sets A and B are *equal* iff $f_A(x) = f_B(x)$ for all x in X.

The *complement* of a fuzzy set A in X is defined by $f_{A'}(x) = 1\text{-}f_A(x)$ for all x in X.

A is a subset of B iff $f_A(x) \leqslant f_B(x)$ for all x in X.

The *union* of A and B is defined by $f_U(x) = \max(f_A(x), f_B(x))$ for all x in X.

The *intersection* of A and B is defined by $f_I(x) = \min(f_A(x), f_B(x))$ for all x in X.

As defined above, A and B are *disjoint* iff $f_I(x)$ is zero for all x in X.

An n-ary *fuzzy relation* is a fuzzy set in $\overset{n}{\underset{i}{\times}} X$.(From Zadeh 1965, p 340–341, p 345–346.)

We can obtain extensions of fuzzy sets in several not quite equivalent manners (Zadeh 1968, p 97):

(a) Probabilistically by sampling, letting the computed grade of membership value for each element be its sampling weight;

(b) By a threshold, rejecting all elements with membership values below a chosen threshold.

(c) By any other selection procedure we can think of and which makes use of the grade-of-membership values.

3. Systems and Automata

The concepts *system* and *automaton* as defined in this chapter are abstract entities and theoretical constructs, not descriptions of empirical phenomena. But they will eventually be usable as descriptors of empirical phenomena.

3.1. Systems and Processes

(3.1.1) Every *system*, A, is characterized by three sets of values, collectively called *state repertoires*:
a set called *input variable* or *the set of A's external circumstances;* a set called *state variable* or *the set of A's internal states;* and
a set called the *output variable* or *the set of A's manifest states.*

(3.1.1a) Remark: Throughout this chapter shall "X" denote the input variable of A, "Y" A's output variable, and "S" A's state variable. We assume also that $X=\{x_i\}$, $Y=\{y_j\}$, $S=\{s_k\}$.

(3.1.2) At any sampled time point, t, is A characterized by at least one element from the set $(X \times S)\times(S \times Y)$. We assume A namely to be simply a relation between $X \times S$ and $S \times Y$ which holds for some unbroken sequence of discrete time points $t = t_1,t_2,\ldots t_m$. During the time interval (t_1, t_m) we shall then furthermore claim that $\{((x_t,s_t), (s_{t+1}, y_t))\} \subset (X \times S)\times(S \times Y)$. Every structure on $(X \times S)\times(S \times Y)$ can thus also be called a system; if m exceeds 1, we say that A is a *dynamic* system; if $m = 1$, we call A *non-dynamic* or *static.*

(3.1.2.1) The elements $((x_t,s_t),(s_{t+1},y_t))$ are called *A's behavior at time point t.*

(3.1.2.2) Often one uses also a slightly different notation for the characterization of A, f.i.

$$X \times S \rightarrow S \times Y$$

or

$$(x_t,s_t) \rightsquigarrow (s_{t+1},y_t)$$

or

$$(S_{t+1},y_t) := A(x_t,s_t)$$

or

$$A(s_{t+1},y_t|x_t,s_t)$$

where the arrows or the letter "A" explicitly denote the structuring relation.

(3.1.2.3) "*Process*" is in this chapter taken to be synonymous with "dynamic system". Later we shall distinguish between processes and the systems that *generate* these processes (cf. section 6).

3.2. Automata or Mechanisms

(3.2.1) Unambigous structures on $(X \times S) \times (S \times Y)$ are called *mechanisms*, or *determinate* or *realizable systems*, or also *automata* or *programs*. (In later chapters, and in analogy to the distinction between systems and processes, we shall distinguish between automata and the programs by which automata behavior can be described. But in this chapter are automata preliminarily assumed to be program-executing devices which are completely transparent and faultless.)

(3.2.2) Automata with small repertoires are often defined with the help of socalled *automaton tables*. Assume, for instance, the automaton A to be the set $\{((0,0),(0,0)),((0,1),(1,0)),((1,0),(1,0)),((1,1)(1,1))\}$. A can then be more transparently described by the table

	s_t	
x_t	1	0
1	(1,1)	(1,0)
0	(1,0)	(0,0)

We read this table as follows:

If x_t, the value of the input variable at the sampled time point t, is $= 1$ and if simultaneously s_t, the value of the state variable S at time t, is $= 0$, then is $(s_{t+1}, t_y) = (1,0)$; that is: the value of S will change to 1 and thus be 1 at time $t+1$, while the output of A at t will be 0.

(3.2.3) It is easy to summarize the behavior of a small automaton in an automaton table. Let, f.i., the following table be given:

	1	0	−1
+	(1, yes)	(1, perhaps)	(0, no)
*	(0, yes)	(0, perhaps)	(0, no)
−	(0, yes)	(−1, no)	(−1, no)

Furthermore, assume that the following sequence of inputs values also is given.

t	1837	1845	1872	1910	1914	1933
X	+	+	*	–	*	–

Let finally s_{1837} be 0. What then is the behavior of the described automation during the time interval (1837, 1933)?
The answer is:
$(x_{1837}, s_{1837}) = (+, 0)$ is associated with $(s_{1845}, y_{1837}) = (1, \text{perhaps})$. (x_{1845}, s_{1845}) is consequently (+, 1), which, according to the table, is associated with $(s_{1872}, y_{1845}) = (1, \text{yes})$; and so on. The complete answer is:

time t	external circumstances X	internal states S	external states Y
1837	+	0	perhaps
1845	+	1	yes
1872	*	1	yes
1910	–	0	no
1914	*	–1	no
1933	–	0	no

A list of this format is called a *protocol* of the automaton's behavior.

(3.2.4) The other way around can we try to construct an automaton table based upon a protocol. It can now, of course, happen that the table remains incomplete. (Empirical research can be viewed as a constant attempt to derive parts of automata tables, 'laws', from observation protocols.) Let the following table be given:

t	X	S	Y
x–8	gib	1	ooh
x–7	gab	1/2	aah
x–6	girk	1/2	heavens
x–5	goke boinggk	1/4	never again
x–4	6 aspirins	0	–
x–3	gab	1/4	–
x–2	gab	1	–
x–1	girk	1	mmm

The table which we can derive from this protocol is incomplete since not all combinations of values have been observed or occurred:

	1	1/2	1/4	0
gib	(1/2, ooh)	?	?	?
gab	(1, –)	(1/2, aah)	(1, –)	?
girk	(? , mmm)	(1/4, heavens)	?	?
goke boinggk	?	?	(0, never again)	?
6 aspirins	?	?	?	(1/4, –)

3.3. Coupled Automata

3.3.1. Unidirectionally Coupled Automata

Assume the existence of two automata, A and B, where the input variable of B is identical with the output variable of A. For instance,

A =

	a_1	a_2
e_1	(a_2, d_1)	(a_1, d_2)
e_2	(a_1, d_2)	(a_2, d_1)

B =

	b_1	b_2	b_3
d_1	(b_1, c_1)	(b_1, c_2)	(b_2, c_1)
d_2	(b_2, c_2)	(b_3, c_1)	(b_3, c_2)

In this case is B said to be *coupled to* A; we denote this by "A → B". B being coupled to A implies evidently that the behavior of A can influence B, and that A's external circumstances (inputs) are not irrelevant to B.

We shall illustrate this with the help of two protocols:
Protocol 1: The initial values, at t=1, happen to be e1, a1, and b1, and A is subjected to the input sequence e1, e2, e2, e1, e2:

t					
1	e1	a1	d1	b1	c1
2	e2	a2	d1	b1	c1
3	e2	a2	d1	b1	c1
4	e1	a2	d2	b1	c2
5	e2	a1	d2	b2	c1
6		a1		b3	

Protocol 2: The initial values are the same, but A is subjected to the input sequence e1, e1, e2, e2:

t					
1	e1	a1	d1	b1	c1
2	e1	a2	d2	b1	c2
3	e2	a1	d2	b2	c1
4	e2	a1	d2	b3	c2
5		a1		b3	

One can see that the concrete behavior of both A *and* B is dependent upon the inputs to A. We say often more carelessly that B is *dependent* upon A; it is then tacitly understood that it is the *behavior* of B which is dependent upon the *behavior* of A, and that the behavior of A, in turn, is dependent upon the inputs to A.

3.3.2. Mutually Coupled Automata

Assume now that also A is coupled to B, and that $e_1 = c_1$ and $e_2 = c_2$. A is again put into state a1, B into state b1, and the first input to A is again e1. The protocol can now be derived without an input sequence being given:
Protocol 3:

t					
1	e1	a1	d1	b1	c1
2	e1	a2	d2	b1	c2
3	e2	a1	d2	b2	c1
4	e1	a1	d1	b3	c1
5	e1	a2	d2	b2	c1
6		a1		b3	

and so on. Sooner and later will the protocol arrive at a line which is, except for the time indication, identical with an earlier line; from then on the protocol will repeat itself, again and again.

A and B are here said to be *feedback coupled via* each other. (The *feedback* is the output of the other automaton when this output becomes the next input to the first automaton. For instance, take the protocol row for t=3. The feedback upon A's output d2 is B's output c1 in the form of the input e1 at t=4.)

3.3.3. Multiple Couplings

Assume a third automaton, F, with the table

	f1	f2
(c1, d1)	(f1, g1)	(f1, g2)
(c1, d2)	(f2, g1)	(f2, g2)
(c2, d1)	(f1, g1)	(f2, g2)
(c2, d2)	(f1, g2)	(f2, g1)

This automaton is evidently coupled to both A *and* B.

Example (Protocol 4): Let the internal state of F at t=3 be f2. We shall add the protocol of the behavior of F to the protocol 3:

t							
3	e2	a1	d2	b2	c1	f2	g1
4	e1	a1	d1	b3	c1	f2	g2
5	e1	a2	d2	b2	c1	f1	g1
6		a1		b3		f2	

and so on.

(3.3.4) Example: In the sociological context, the modelling of interaction is naturally of special interest and importance. We shall therefore now describe the interaction between two automata in anthropomorphic terms. The resulting model is only intended to be of pedagogical use, but it can also be viewed as belonging to the class of very first and crudest structural models of human interaction. (This example was inspired by Maruyama 1963a.)

Assume the existence of two automata, nicknamed *Antonius* (or *A*) and *Cleopatra* (or *C*). The internal-state repertoire of Antonius is {romantic, happy, frustrated, angry, gentle, sad}, that of Cleopatra is {proud, dis-

gusted, bored, teasing, gentle, angry}. (All internal states will in tables be denoted by their first letters: "a" for "angry", "b" for "bored", "d" for "disgusted", etc.)

We make now the (arbitrary) additional assumption that Antonius and Cleopatra perceive each other different from how they in fact are: For instance, A is *happy*, but C believes incorrectly that A is *proud;* neither A nor C are able to recognize such internal states correctly which they themselves cannot experience. The picture A* of A, formed by Cleopatra, is thus a mapping of A's internal states onto C's internal-state repertoire; for instance:

A	a	f	g	h	r	s
A*	a	d	g	p	g	b

A's picture C* of C is correspondingly, for instance:

C	a	b	d	g	p	t
C*	a	r	f	g	h	h

These two mappings can be elaborated when one has defined external-state repertoires.

The external-state repertoires of A and C are, f.i., {makes jokes, stares into the sky, whistles, walks to and fro, does not answer questions, sings love songs, kills 10 slaves, leaves the palace, embraces C} and {dances, stares into the sky, sends A way, does not answer questions, embraces A, sends for her other lovers, yawns, feeds her monkey}, respectively. But we shall in tables write "1" instead of "makes jokes", "2" instead of "stares into the sky", etc. The external-state repertoires of A and C are, in this notation:
{1, 2, 3, 4, 5, 6, 7, 8, 9} and {10, 2, 11, 5, 12, 13, 14, 15}, respectively.

The automata tables for A and C are presumed to be:
For A:

C* \ A	a	f	g	h	r	s
a	a8	a2	g9	g5	f4	s2
f	h1	f2	g7	g9	g6	s2
g	a7	s2	r6	h3	r9	r9
h	a7	a8	h1	h9	h9	f2
r	f5	s5	r6	h9	h9	s9

For C:

A* \ C	a	b	d	g	p	t
a	a11	t10	d15	g5	t14	b14
b	a13	d14	a2	a5	a11	d15
d	d15	b2	b2	d11	p13	t10
g	p13	b14	g5	p12	p12	g12
p	a13	d13	b15	g12	p10	g10

(these tables are read as follows: if C is in state d and believes A to be in the state A* = a, then she responds with d15, that is: she remains disgusted and she feeds her monkey. Likewise, if A is in state r and he believes C to be in state C* = a, then he responds with f4, that is: he becomes frustated and walks to and fro.)

Now, finally, we shall respecify the apperception mapping $A \rightsquigarrow A^*$ and $C \rightsquigarrow C^*$. We shall namely make the realistic additional assumption that A and C cannot observe each other's internal states directly but only the external states. The apperception mappings are thus $\{1, 2, 3, 4, 5, 6, 7, 8, 9\} \rightsquigarrow A^*$, performed by Cleopatra and $\{10, 2, 11, 5, 12, 13, 14, 15\} \rightsquigarrow C^*$, performed by Antonius:

A*	1 ↓ p	2 ↓ b	3 ↓ a	4 ↓ d	5 ↓ a	6 ↓ g	7 ↓ d	8 ↓ a	9 ↓ p

C*	10 ↓ h	2 ↓ f	11 ↓ a	5 ↓ f	12 ↓ r	13 ↓ f	14 ↓ g	15 ↓ g

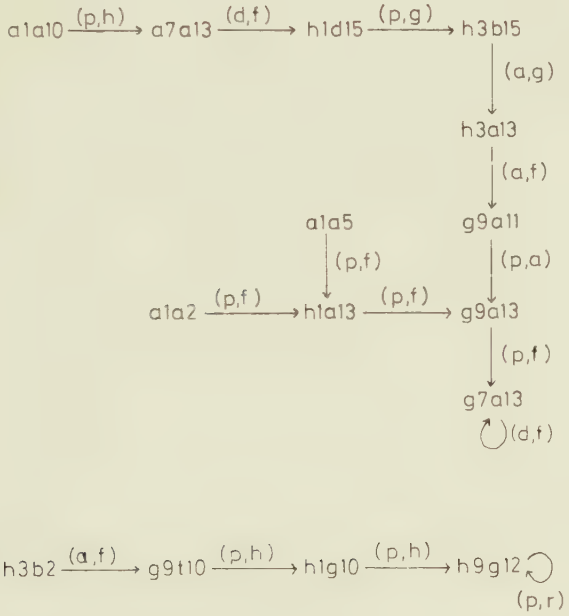
(We see that there are, for a number of reasons, quite many misunderstandings between Antonius and Cleopatra. Does anyone care to derive a *model of interviewings* from this design?)

Let us illustrate the total system's behavior with a part of the 'kinematic' graph $A \rightsquigarrow C$:

We must assume some initial total-system state, A = h3 and C = b2, say. We can at once conclude that Cleopatra will believe that A* = a and Antonius will believe that C* = f. (A = h, C* = f) maps into A = g9 according to the automata table for A, and (C = b, A* = a) maps into C = t10 according to the table for C. We depict this process step in graphical form as

$$h3b2 \xrightarrow{(a,f)} g9t10$$

The complete kinematic graph illustrating the dyad's behavior regardless of starting states is very extensive: we shall in figure 3.3.4 depict only a part of that graph.



3.4. Equilibrium and Multifinality

"Equilibrium" and "stability" are much-used concepts in connection with dynamic systems. A system is in a *state of equilibrium* (with regard to a certain set of system properties) if these properties do not change unless the system is given an 'extraordinary' input. The *degree of stability* of these system properties equals the system's probability of receiving no such extraordinary input.

Now *if* an equilibrial system receives an input which forces the system out of its equilibrial state, it can happen that it eventually returns to the equilibrial state if no further such 'disturbance' occurs. (The classical example is a pendulum we push at.) We can thus associate a set of 'neighboring' internal states with each equilibrial state, this set being closed (in the sense of section 2.6.2b). We say that this closed set of internal states is a *region of equifinality*. The *degree of equifinality* with regard to a given region of equifinality equals the system's probability of being in a state that belongs to this region.

A system is *multifinal* if its repertoire of internal states comprises several regions of equifinality. Multifinality implies that two seemingly quite similar states can lead to essentially different later states if they belong to different regions of equifinality (Maruyama 1963b.)

3.5. Fuzzy Systems

A *fuzzy system* is a fuzzy subset of $(X \times S) \times (S \times Y)$. Each element in the universal relation is extensionally or intensionally characterized by a grade-of-membership value. (Zadeh 1968, p 99.)

Fuzzy systems can be determinate or indeterminate.

4. Multi-Levelled System Description

4.1. System Composition

A system B is *indirectly coupled* to another system D if B is coupled to a system C and C is coupled to D. The couplings B—C and C—D are said to be *direct*.

If there is a non-empty set of systems such that each system in the set is either directly or indirectly, unidirectionally or bidirectionally, coupled to every other system in the set, we call that set of systems a *composed system*. The set members are called *subsystems* (of the composed system) or *system parts*.

The complement of a subsystem is called *environment*. (The environment of a subsystem comprises thus all other subsystems.)

(Remark: The social-science concept *field* (as introduced by Lewin; cf. Mey 1965) denotes a more concrete subclass of composed systems.)

4.1.1. System Structure

We see a *structure* of a composed system if we disregard the definitions of the concrete systems that are the system parts of the composed system, and if we merely pay attention to which subsystems are coupled to which.

We see *a* structure and not *the* structure because we can choose to regard certain coupled systems as one single composed system, and since we are to disregard all subsystem definitions, we cannot distinguish composed from non-composed ('elementary') systems.

But if we choose to 'resolve' all composed systems of which we know how they are composed of elementary systems, we obtain a 'maximally detailed' system structure.

4.2. Levels of Detail

When we shift from regarding a system as composed to ignoring the fact that it is composed, we are said to shift the *level of detail*. The level of detail is said to be *higher* while we speak of the set of subsystems of the composed system, and *lower* while we abstract from the set to a single system. (Instead of "level of detail" one says also "resolution level". "Level

of abstraction" or "description level" are further synonyms, but in connection with these latter one says that the level is higher when it is abstracted, lower when it is detailed.)

4.2.1. Reality, Science, and Description Levels: An Example

It has been suggested by many (for instance, Bertalanffy 1968 (despite warnings against reductionism, such as on p 87–88); Weiss 1971a; Gerard 1964; Laszlo 1972; to name just a few) more or less explicitly that the various scientific disciplines describe reality on different levels of abstraction. Such a view implies, for example, that psychology is just another way of looking at physiological processes; biology is an abstracted chemistry; chemistry, in turn, is abstracted physics. Sociology is abstracted psychology, and economics abstracted sociology.

In fact, we find in all disciplines such ideas of description levels; the more abstract level is usually called *molecular* or *macro* level, the less abstract *molar* or *micro* level.

(Cf. Mulckhuyse 1961.)

4.3. Input and Link Abstractions

When speaking about systems in a less detailed manner, one usually abstracts from the set of most frequently occurring inputs and calls that quotient set 'no input'. An input is then, in consequence of that convention, always something *extraordinary*. We are in the social-science context mainly concerned with multi-final systems, and it is therefore easy for us to stipulate the following explication of "no input": "no input" means that no input is received which forces the system out of its most usual region of equifinality.

It is then an obvious extension of this convention to say that 'no links' (or 'no couplings') exist between two systems A and B as long as no inputs (in the sense specified above) are 'transmitted' from A to B. (A certain 'hysteresis' may be, and is, assumed after each 'transmittal' of an input. That means: As long as inputs are 'transmitted' with some regularity, the link is assumed to be permanent.)

4.4. Morphogenesis

'Morphogenesis' (Maruyama 1963b; Buckley 1967, p 58–66) is perhaps the most fascinating of the new general concepts introduced into scientific thinking by cybernetics. It denotes the propagation (among the subsystems of a composed system) of the effects of a single (or a few) state or output changes. Keeping the recently (in section 4.3) stipulated conventions in mind, this must mean that at least one subsystem has received an input which forces it out of its usual region of equifinality into another one, and that it also generates outputs which force other subsystems out of *their* regions of equifinality into other ones, etc., in a chain-reaction manner. (Cf. Karlsson 1958 on Markovian (stochastic) models of various morphogenic processes in social settings.)

The changes of a subsystem to a new region of equifinality can on a less detailed level be described in terms of a socalled *step function* (if regions have been abstracted into single states). Since such 'step changes' are from one region to another, they are permanent until a new 'step change' occurs.

4.4.1. Quantitative vs. Qualitative Changes

Observe that 'step changes' and state changes of the kind we hitherto have talked about, are one and the same. Similarly, functions and 'step functions' are the same. "Step" is merely added to clarify the perspective one has chosen: A continuous change, if looked upon from a macro perspective, can seem to have been a discrete 'step change'. Moving out from one region and into another *is* a continuous change, but if sampled observation - time points are sufficiently wide apart, the system is found to be in quite different states.

For instance, assume that an electron breaks out of its present trajectory and flies off (and thus 'changes' the atom into a positive ion). This electron will not disappear at one place and then appear at another (unless the observer blinked), but it will move smoothly along a new, although less predictable, trajectory. The interesting point, however, is that *one* of the continuous (infinitely small) position changes in the given field of forces happens to imply the transition from one region of equifinality (namely out of the cycle of positions that constitute an electron's trajectory around the nucleus) to another (into one of the innumerable multitude of trajectories that lead away from the nucleus and of which we do not know definite 'goal states').

4.4.2. The Growth of Change

A subsystem can be influenced many times in the course of a particular morphogenic process and thus *accumulate* effects (that is, in another terminology: it can become increasingly *intensive* with regard to some aspect each time it is influenced), and the 'intensity' of the subsystem with regard to an internal-state aspect can also manifest itself in the choice of outputs. (For instance, a more intensive ('convinced') Marxist will tend to behave differently from a less convinced one.)

We can here safely paraphrase the system-behavior idea such that the 'choice' of behavior by a system depends upon its environment. If the environment changes, the system can be expected to behave differently from before. (In the human context, for instance: Things that could not be done before, are now possible; or things that were attractive before, have now become unattractive; and people change their behavior in consequence, either quietly (they adapt, avoid, or use the occasion) or in counter-action (they protest, boycott, destroy, or organize).) The world can be interpreted as an 'eternal' morphogenic process: Changes in states and behavior constitute continuously new environmental givens and thus evoke new changes in states and behavior, et cetera.

4.4.3. The Growth of Structure

Output changes can imply the establishment of new couplings between systems, and such couplings can, in turn, influence system behavior essentially. Further couplings can thus become more improbable, but the opposite can also be the result, namely that further couplings become more and more probable, the greater the number of systems that already have directly or indirectly become coupled to the first.

Such structure-synthesis processes in aggregates of systems are often called *self-organization processes* (cf. Yovits/Cameron 1960; Foerster/Zopf 1962; also Volume 1, section 5).

4.4.4. Remark

Morphogenesis can only be modelled in aggregates of systems (or, equivalently, a composed system), but not in a single 'black box'. Morphogenesis is, in the social context, of utter importance. Thus, if one wishes to obtain an adequate society model, one *cannot* hope to implement it in the form of a behavior model. Econometrists try anyway and find that they need new data continuously for aligning their models with reality. An adequate morphogenic model, in contrast, will *itself* generate the state ('parameter') changes which make such alignments more rarely necessary. That this must be so is obvious: if all system components are adequately

modelled, the modelling must comprise rules for each component how it is to adapt to various classes of other components. Now, a morphogenic process in reality is brought about by recursive adaptations of systems to new environmental givens; a morphogenic process in the model will then likewise imply recursive adaptations of modelled systems to changed givens.

4.5. System Definitions

The definition of a system in terms of a set of quadruples (cf. section 3.1.2) is an *extensional* definition because it explicitly specifies the *set* of all elements referred to in the definition.

An *intensional* definition of something specifies instead a *rule* with the help of which we should be able to recognize with regard to anything presented to us whether it belongs to the extension of the defined something or whether it does not (cf. sections 2.2.3 and 2.3.1.2). Such a rule can, by a convention, be abbreviated to a rule name.

Large automaton tables are clumsy, and if one can detect regularities in often-used automaton tables, one will therefore try to elaborate a more concise intensional definition to replace the extensional one. If we write "y:= twice(x)", this is quite evidently more concise than a table for the automaton TWICE and for all the inputs x we can mention in our life time, and yet, it denotes that table and also all other tables for the automaton.

As we said, we can use rule names in intensional definitions of an automaton only if the named rule (an automaton) can be assumed to be known. It is obvious that not all automata have been found to be of sufficient practical importance to obtain a name.

Construct for yourself an automaton table of multiplication by 3 for the integers 1 through 10 as inputs. (Since the internal state is invariant, it is negligible.) You see that you can do it, guided by our intensional definition, because you know what automaton we mean. But you have perhaps seen that automaton before? Take the integers 379, 413, 562, 708, 855 as inputs instead; you have surely never seen that automaton before, and yet, you know how to construct its table. How come?

Well, it is because you know a general intensional definition of multiplication of any input by any value. That definition is usually even *operational*; it tells you not only how to recognize an automaton-table element, it even tells you how to construct the table if you have been given the inputs.

Most systems for which there are generally known operational definitions (for instance, the automata y:=twice(x), y:=half(x), z:=percent(x,y)), have invariant internal states (and can thus be regarded as static). They are probably well-known *because* they are especially simple, and sufficiently frequently needed to be thought worthwhile to remember. (But do you happen to know how to compute degrees Réaumur from degrees Fahrenheit, for instance?)

4.5.1. The Use of Partial Intensional Characterizations

The properties of relations we spoke of earlier (cf. section 2.6) are examples of intensional characterizations that are not complete relation or system definitions. But if one combines several such partial intensional characterizations, one can screen off a great number of systems.

For instance, a system we think of can be given all these intensional characterizations: it is dynamic, it has two internal states, it has two inputs, it is determinate, it is not closed, it is disconnected, it is irreflexive, it is assymetric, it is intransitive. Can any of the systems defined in section 3 be the one we mean?

5. Abstractions, Simplifications, and Models

There is reason to believe that all apperception and thinking employs models (cf. Stachowiak 1973, p 56–57). In the elementary cases, models are simply representations; and non-elementary models are to be understood as systems composed of elementary models.

We have claimed earlier that we will never be able to compare our models with an unmodelled reality, to find out how close we are to having a 'true picture' of it. All we can do is to compare various models, more or less abstract and comprehensive, with one another.

Models can be more or less *similar* to other ones. We distinguish two types of similarity in models: similarity of behavior, and similarity of com position. In consequence, if two models are similar in behavior, we say that they are *behavior models* of one another, while if they are similar in composition, we say that they are *composition models* of each other.

Observe that "similarity" cannot have the meaning of "total similarity" here, and that we thus cannot expect that two models are similar *in all respects*. On the contrary, (and in analogy to our definition of "abstraction", which referred to a specified equivalence relation): "similarity" will in our texts only mean similarity with regard to certain aspects of behavior or of composition.

(Relevant literature: Apostel 1961; Klir/Valach 1967; Stachowiak 1973; Frey 1967, p 85 ff.)

5.1. Behavior Models

A system M, under the circumstances N, can be regarded as being a behavior model of a prototype system P (being under the circumstances Q) iff P's behavior under Q is *similar* to M's behavior under N.

M and P are said to manifest *similar behavior* if

(a) there is a single-valued correspondence between the input set of P and the input set of M; and

(b) there is a single-valued correspondence L between the outputs of P and those of M; and

(c) $y := P(x)$ implies $L(y) := M(K(x))$; that is, the input-output relation x y in P corresponds to an input-output relation w z in M, where $w := K(x)$ and $z := L(y)$.

We say that M is a *behavioral simplification* of P if the range of L has fewer elements than that of P, and/or if the domain of K has fewer elements than that of P.

Behavior Models are also called *analytical models.*

5.1.1. Example

Let P = {(x,y)} = {(1,a),(2,g),(2,b),(3,c)} and

M = {(w,z)} = {(red,win), (black,lose), (green,lose), (blue,lose), (yellow,win)}.

Let futhermore

K = {(x,w)} = {(1,red),(2,black),(3,red)}, and

L = {(y,z)} = {(a,win),(b,lose),(c,win),(g,lose)}.

Now, if c:=P(3) according to the definition of P, there must be a correspondence L(c):=M(K(3)) if M is to be a behavior model of P. We find that L(c) = win and K(3) = red. We expect thus "c:=P(3)" to correspond to "win:=M(red)", which according to M's definition is the case.

If all the single transitions in P have such a unique correspondence among the transitions in M, M is a behavior model of P (assuming the correpondences K and L).

By the way, M is a behavioral simplification of P.

5.2. Composition Models

Behavior models are the simplest models. They are superficial insofar as they disregard differences in how the compared systems are composed of subsystems.

Composition models imply behavior models, but they also show similarities in their compositions.

We add therefore to the criteria a,b,c, named in section 5.1, two more:

(d) there is a single-valued correspondence, H, between the subsystems of P and those of M such that if B is any subsystem of P and N is the subsystem which in M corresponds to B according to H, then must N be a behavior model of B;

(e) for any subsystems A and B of P, if A→B holds, then H(A)→H(B) must also hold in M.

Rule e looks more severe than it is. It merely requires that we see to it that all input and output labels used in the definitions of C's subsystems receive *unique* correspondences in D. The mapping of couplings is then implicit in the mapping H.

We say that M is a *compositional simplification* of P if the range of H

has fewer elements than the domain of H.

M is a behavioral simplification of P if any of its subsystems is a behavioral simplification of a subsystem of P.

5.2.1. An Example

Let C be a prototype system composed of P, as defined in section 5.1.1, and two other subsystems, Q and R.

Let D be a proposed model of C, composed of M, as defined in section 5.1.1, and two other subsystems, E and F.

Assume that we have shown that M is a behavior model of P, and likewise that E is a behavior model of Q, and that F is a behavior model of R.

Remains only to find a correspondence H={(P,M),(Q,E),(R,F)} such that rule e is observed, and P is shown to be a composition model of C (assuming the correspondences H, K, L, etc.).

By the way, D is not a compositional simplification of C, but it is a behavioral simplification since at least M is.

5.3. Ranking of Models

There is no unique way to rank models. The ranking depends upon whether we seek the simplest model, the most comprehensive one, the most detailed, etc. Probably most often we seek the model that simultaneously is most 'valid', most comprehensive, and most detailed—in that order of importance. Assume that a model is 'valid' if we have shown that it is a composition model. If two models are both valid, the more comprehensive is the 'better' one. A model M is more comprehensive than another model N if the number of elementary subsystems of M exceeds the corresponding number N, presumed that the elementary subsystems have been defined on the same level of detail in both systems. If this latter presumption is not fulfilled, we count the more detailed system's number of subsystems on the elementary level of detail of the other system. If the number of subsystems should happen to be equal, the more detailed model is obviously the 'better' one.

In order to be able to make such a comparison, it is necessary, of course, that M and N be models of the same prototype.

6. The System Embodiment

If we say that an organism is a system, we must not forget that it is *we* who draw an arbitrary boundary between certain systems and other systems; of some of these systems we say that they are subsystems of the organism, of the others we say that they are parts of the organism's environment. And also the subsystems are arbitrarily delimited. et cetera.

Every system is, in that sense, an artefact or fictive. That is: all systems are models (in the indicated sense). But most often when we speak of a certain empirical system, we wish to denote the model's *prototype*, not the very model that we have in our mind.

This prototype can also be said to be the system's *embodiment* if one wishes to be quite unambiguous. What we have called 'system' in section 3, on the other hand, can now be discovered to be merely the modelled prototype's behavior definition. This definition can also be called *program* or *(orded set of) operating rules*.

We hope that it is now evident how our remarks in sections 3.1.2.3 and 3.2.1 are to be interpreted: "System" is usually ambiguously used. "Model", "embodiment", and "program" are the three possible underlying meanings. "Process" denotes similarly sometimes a program, sometimes the behavior generated according to that program.

The context of discourse disambiguates fortunately these ambiguous denotations in most cases.

6.1. The Embodiment of States

If a material system has several possible internal states, these must manifest themselves materially, ultimately as changes in spatial positions and/or as changes in energy potentials (and these changes can, in turn, imply changes in observability and/or in classification by an observer).

We can thus say that if a system has more than one possible internal state, it is an abstraction of a system composed of subsystems, while a system with only one invariant state must be elementary.

An elementary system can be said to have different outputs only by shifting its location in space, or its movement in space (including spins), or by 'linking' with different other systems (what 'linking' means must be

analyzed below). Evidently, the very same phenomena could also have been called 'changes in internal state'. As a matter of fact, we can still call *some* of them internal and *some* of them output, if they namely can co occur independently.

Well, what does "linking" or "coupling" mean *ultimately* and *materially*? It seems that "A is linked with B" cannot but mean something like "A is spatially close enough to be under predominant energetic attraction of B". Since this attraction is temporally invariant and, in principle, infinite in its reach in all 3 spatial dimensions, with its maximum at the location of A, it is only the relative proximity of A to B, as compared with other attraction sources, which is important.

(Cf. McCarthy/Hayes 1969, p 470 ff; Mulckhuyse 1961; Bunge 1959; Fuchs-Kittowski 1969.)

7. Communication and Control Processes

Control processes are processes of communication of composed systems with their environments for the purposes of evoking or avoiding the formation of specific environment states ('situations'). Such communications can be unidirectional (also called 'open-loop'), bidirectional (also called 'closed loop'), or multidirectional. (These process structures correspond, of course, to the 3 types of coupling we have discussed in section 3.3.)

7.1. System Behavior and Communication

The famous communication-chain paradigm by Shannon (Shannon/Weaver 1949, p 6) speaks of a *message*, transformed by a coding device ('transducer') into a *signal sequence*, being sent by a *source* along a *communication channel* to a *destination*, where the signal is retransformed by a decoding device (the *receiver*) into a message. This set of necessary communication-chain components is completed by an in practice unwanted but unavoidable component, namely a *noise* source, which sends 'competing' messages (in the form of signal sequences) along the channel. (The noise is to be understood as an abstraction from innumerous and uncontrollable transmission-disturbance sources.)

This communication chain is, of course, a composed system, and each system part can itself be composed. The chain-like system structure suggests that all system parts are unidirectionally coupled (but that is usually a simplification).

Observe that channels naturally are systems although they in diagrams commonly are represented only as arrows while all other chain components are represented as boxes.

The communication-chain paradigm is an example of subsystem differentiations according to ascribed purposes. The source, the transducer, the channel, the receiver, the destination, and the noise source, they all are 'quite objectively' merely systems that behave. *Any* system is a message source, and a noise source, and a transducer, and a channel, and a receiver, and a destination. It is only *we* who sometimes interpret them as this, sometimes as that.

The communication chain is a simplification also in its being 'self-sufficient': The sources receive no inputs and the destination produces no output. But communication processes are 'in reality' most often parts of control processes where messages are sent for the *purpose* of influencing some part of the environment. (Or, at least, such purposes can often be 'read into' observed system interactions in nature.) Now, only if we have assumed a communication purpose can we distinguish between what is a message and what is noise.

(Cf. section 1.7.2.)

7.2. Reaction Delays

We have hitherto only discussed synchronized subsystem behavior, that is, we have not taken into account that composed systems may manifest reaction delays (or transmission delays if we choose to regard them as channels). Some systems can react fast, some very slowly, and such reaction-speed characteristics are important.

That means, if we change level of detail with respect to system definition, we must also change time steps. The transmission time must not change if we describe the chain A→B→C→D→E as A→F→E, where F is assumed to be the composed system B→C→D. F shows delayed reaction as long as we regard it as a 'black box'.

Transmission speeds or reaction delays are of essential importance in all control processes, and it is therefore often necessary to go into composition details only in order to obtain precision regarding reaction times.

7.3. Goals and Regulators

When we describe systems in purposive terms, we usually distinguish system states which we call *goal states* and/or certain states we call *avoidance states*. A study of a system's behavior rules tells us then which system inputs are to be avoided or attractive in order to attain or maintain a goal state and/or not to attain or maintain an avoidance state.

Often we cannot change the system's environment by simply redefining it, or by removing undesirable parts of it, but we may be able to add new parts or to change couplings. We need in that case a *regulator* which we couple between the system and its environment. A regulator is a system which happens to be able to transduce unfavorable inputs into favorable ones. (See Ashby 1956, Part Three, for the operational details.)

The process of transducing unfavorable into favorable influences is called *regulation*. We can distinguish three types of regulations: '*by flight*', that is, by interrupting the coupling between the system and certain parts of the environment; '*by attack*', that is, by changing the states of certain parts of the environment; and '*by adaptation*', that is, by changing the internal state of the system such that it is prepared for the environmental input; to be more explicit: the system is induced to go to a state in which it will react such that it remains within the present region of equifinality when disturbed by the expected input.

All of these regulations either require a certain continuity in environment state changes, which allows the system to extrapolate trends of change, *or* the regulator comprises an environment model and can thus predict environment behavior, *or* the regulator receives forewarnings of environment states to be expected and can act fast enough to compensate before or as they begin to influence the system of concern.

(Relevant literature: Anschütz 1967; DiStefano et al. 1967; Ashby 1956, Part Three.)

7.3.1. Decomposition and Survival

A composed system is said to *decompose* if certain subsystems become independent in their behavior upon the inputs from the rest of the subsystems. A simple example: Assume that the following table defines a subsystem that is coupled to two other subsystems:

	a	b
(0,0)	(a,1)	(b,0)
(0,1)	(b,0)	(b,0)
(1,0)	(a,1)	(b,0)
(1,1)	(a,1)	(b,0)

As long as this subsystem is in state a, it is input dependent, but once it has received the input (0,1), and thus gone to state b, it has become input independent.

(Composed-system state changes which are such that one or several subsystems become input independent, could be called *fatal* (for the composed system), and the composed system could be said to *survive* as long as no fatal state change has occurred. But such a convention seems impractical in view of the everyday-language meanings of these concepts and the fact that there are decompositions of biological organisms that are not lethal (such as when a tree sheds its leafs), sometimes even the contrary, in

identical reproduction processes, for instance. We shall therefore introduce the concept "intrinsic regulator" and then define "survives" for intrinsic regulators only.)

An *intrinsic regulator* is a system which happens to regulate against its own environment in such manner that inputs from the environment predominantly are *tolerable*.

Inputs to an intrinsic regulator are called *tolerable* if the system's ability to prevent intolerable inputs in the future is not impaired by them.

We say that the intrinsic regulator *survives* an input if the system's abili ty to prevent intolerable inputs is not permanently removed by that input. We must presuppose that some ultimate intolerable inputs are known.

Part Three

Epistemological Processes

Part Three tries to develop a theory of scientific theorizing. We shall first postulate that each human being develops a theory of data validity, basically all by himself; we call this theory the *common-sense theory of data validity* because we believe that it can be found in all 'normal' (that is, statistically normal) people. We elaborate then the concept "theory" (in section 9.4) and try to give a pragmatic justification for theorizing (in section 9.1, 9.2 and 9.3). Section 10 explicates what distinguishes scientific from non-scientific theorizing and section 11 deals with the problem of the 'correctness' of dynamic models. Section 12 looks at dynamic modelling from the point of view of computation: it is not only necessary to have a dynamic model's definition, it is necessary to *work* with it. Since this can be very tedious, we let a computer do the 'stupid' computational work, which frees us to do other things. The last section, section 13, is an outlook into the future of computer-aided science.

8. Common-Sense Theory of Data Validity

Man's common-sense theory of data validity (which is basic to all scientific endeavors) is the result of individual experiences from interactions with the environment. It is the first theory he develops in his life.

We cannot but assume that the very first data are 'spontaneous' representations of attribute relations in the human mind and that propositions involving these data are consequently formed. We adopt a pragmatistic view here and assume that the infant somehow learns that these propositions regarding data which together form a theory regarding object permanence in perception (and other such basic perception structuring theories (f.i., regarding causality) which we from our a posteriori view judge to be correct) are better fitted to enable intended evokations of positive reactions from the environment and intended avoidances of negative ones.

Unfortunately, we do not yet know sufficiently about how infants learn to account for object permanence, and we are therefore not yet able to reproduce such learning in computer models. But there seems to be good evidence for our pragmatic view in Piaget's large production of publications on such topics, along with the implausibility of the assumption that perceptive abilities are 'preformed' in the genetic code and that they would emerge even if the infant had no training.

A person's basic common-sense theory of data validity biases him towards acceptance and rejection of theories about reality. In the choice between believing that a table is a solid object or that it is a system of interacting forces, he will spontaneously accept the first, but ask for reasons why the second view can be held.

Data reliability is a prerequisite for data validity. Data-reliability judgments are based upon observations of circumstances of perception, and influenced by the frequencies of perceptions under varied circumstances. (The assessment of perception reliability is part of what usually is called apperception.)

8.1. A Tentative Behavior Model

We can model the 'data-reliability learning' process as follows: In the beginning we know nothing. Let the fact that we are completely ignorant about whether a claim p about a *single* datum is true or not, be symbolized by "(1,0,0)". Whenever we happen to observe that p is true, we add 1 to the second value in the triplet. Whenever we happen to observe that p is false, we add 1 to the last value in the triplet. Thus, assume that we observe first that p is false, then true, then false. The associated triplets will then be after each observation: (1,0,1),(1,1,1),(1,1,2). The reliability of our claim that p is false is now computed as 2/4=0.5. In general, if the triplet is (a,b,c), the reliability of p being false is c/(a+b+c). (The value a/(a+b+c) will be called *the remaining doubt*.)

For reasons which will be evident by then, we shall later (in section 10) stipulate that scientific claims need to have reliabilities that exceed the value 0.5. This stipulation implies that a single observation, according to our model, is not sufficiently reliable to be acceptable as a scientific claim or fact, only as a suggestion.)

We obtain in this manner a *measure of data reliability*. Such a measure can be given more or less rigorous standards. Besides demanding a multiplicity of observations only, one can furthermore demand a multiplicity of observers and/or of observation situations (including measurement instruments used), in order to assess the generality of the observed phenomenon.

8.2. Validity and Reliability

Validity is a more severe demand on data, propositions, or models, than reliability. Validity refers namely also to the intention behind a descriptive statement; such a statement is valid if it 'really' describes what the statement maker intended to describe when he formulated it. If he consistently described something else than he believed he did, he can obtain reliability, and still lack validity.

Demands for multiplicities of observers and multiplicities of observation situations with regard to data collection can be interpreted as attempts to ensure also data validity beyond data reliability only.

(Cf. section 11.)

9. Belief Systems and Theories

We shall now try to show that the systematization of beliefs is necessary for the derivation of adequate criteria for the choice of behavior. Section 9.1 is to show that criteria are necessary for choosing, while section 9.2 will discuss the relationship between goals and criteria. The rest of section 9 deals with truth, beliefs, and theorizing.

9.1. Goals and Decisions

9.1.1. The Principle of Alarity

It seems impossible to us not to be pragmatic regarding the role of *beliefs* (that is our common name for data, propositions, and theories) in everyday life. Science is a process of systematization of beliefs; religion is another such process. Such systematizations are sought for the purpose of decision-facilitation:

People seek namely to behave on the whole at least approximately rational.

(We abbreviate "at least approximately rational" to "alar" and "at-least-approximate rationality" to "alarity". "Alary" is to mean "at least approximately rationally".)

An alar choice between two or several alternatives can be made if and only if choice criteria are given to the choice-maker.

Choice criteria can be values, attitudes, or rules for how to achieve a choice (f.i., by throwing dice, or the exact procedure for looking up random-numbers in a table together with a specification of correspondences between obtained numbers and the rejection or acceptance of certain alternatives).

9.1.2. Goals and Subgoals

If we have a goal (call it G) which we wish to reach in order to be able to achieve another goal (call it H) more easily, more quickly, more securely, or at all, then we say that G is a *subgoal* of H and that H is an 'ancestor' of, or superior to, G.

This explication suggests that *alar choices amongst alternative goals cannot be made if not at least one goal is among the choice criteria.*

Alar choices amongst alternative goals which are to be subgoals of another goal cannot be made if that other goal is not known to the choice-maker at the time of choice-making. That is obvious. But the above suggestion implies more than that: It assumes that *alar choices amongst goals are not made except for the purpose of selecting a subgoal to some goal.* Or to turn the argument around:
A goal which is not a subgoal of another goal cannot be chosen in an alar manner.

Such goals which are not subgoals of any other goals are called 'intrinsic' or 'ultimate'. If there are any such goals—the goals of survival or of pain-avoidance are often cited as examples of intrinsic goals—, then we must have obtained them in a non-alar manner. For instance, we could be born with them, or we could have chosen them on irrational bases.

The fact that intrinsic goals cannot be chosen in an alar manner does not imply that it cannot be an alar goal to find out (in an alar manner) whether there are intrinsic goals for one's behavior. People's eternal questions about the meaning of life imply attempts at finding out whether there is an ultimate goal which can be set up as the ultimate criterium for choosing one's way of life. It is an experience from everyday life that people who believe to have found out that there is an ultimate goal to life, and that they have been able to identify it, often are far less uncertain when it comes to judging various phenomena, at least if they believe their intrinsic goals to be such as stipulated by one of the various religious, philosophical, or political belief systems. These belief systems elaborate namely more or less explicit guidance rules for how to behave adequately with respect to the assumed ultimate goal. The 'religious instinct' that has often been postulated to exist seems to us to be of the same quality and importance as the 'curiosity drive' for the improvement of human alar regulatory capacities.

Science is also a belief system. The ultimate goal it preliminarily stipulates for man has been the discovery of absolute truths. Many men have made this the ultimate goal of their lives and they have consequently sometimes found it adequate to live in ways that were quite incomprehensible to uninitiated observers.

But we said that the stipulation of an ultimate goal for science is preliminary. Science does, in fact, not exclude the possibility that the 'real' ultimate goal of man is or ought to be another one. But assume that a conception of man as an intrinsic regulator (see section 7.3.1) is scientifically acceptable and consequently part of science, can we then say that it suggests that survival needs to be the ultimate goal of man? No, we cannot. We can namely still claim that we wish to survive in order to be able to

discover truth.

Our argumentation in section 9.2 will be the other way around: namely that knowledge (truth) facilitates survival. We do not thereby wish to reject stipulations of ultimate goals that have survival as subgoals.

(Remark: The relationship between non-scientific and scientific belief systems will be discussed later in section 9 when we discuss belief-system coherence.)

It is *practical* to have an intrinsic goal or an ultimate criterium because it allows us to stop doubting, thinking, and hesitating, at a given 'ultimate' point, instead of going on with new instances of 'why?' and 'for what purpose?'. Religions and political ideologies have specifically been elaborated for the purpose of furnishing relatively simple evaluation and decision rules for practical use in the context of everyday-life situations.

"Women or heathens have no souls", or "all employers in this country are capitalist exploiters", or "pain is maya", or "it is wrong to kill" are examples of simple correspondence rules between theory constituents and parts or states of reality, to be found in political and religious belief systems. Being given such correspondence rules plus a decision algorithm in theoretical terms makes it understandably easy to motivate one's decisions (to cite Robinson's Cardinal as merely one example) and to 'debunk' one's adversaries.

(Relevant literature: Simon 1957 and March/Simon 1958, two books that have influenced us profoundly. Cf. also Keiler 1970.)

9.2. Survival and Truth

Let us assume that human beings are intrinsic regulators.

It is obvious that a human regulator needs to be able to choose among alternative interpretations of the world and among alternative regulation strategies. A human regulator must therefore comprise a decision-making system (besides a perceptor system and an executor system). The precision of regulation (meaning "the degree of congruence of the intended with the factual result of a regulation effort") depends evidently upon the precision, validity, and completeness of information which the regulating agent has regarding the state and structure of his environment.

It is therefore rationally adequate for man to seek to elaborate a precise, valid, and complete theory of reality.

9.3. The Relativity of Truth

Bertrand Russell argues in his *The Problems of Philosophy* (Russell 1962) that although we can never know about any isolated datum whether it is true, we can often judge its plausibility if we base our judgment upon a theory. The obvious question, of course, is where this referenced theory comes from.

Similarly, scientific epistemology is obviously 'biting its tail'. It discusses the definition of truth and criteria of truth, it accepts some views and rejects others, and it does so on the basis of logic which itself is a theory concerning truth. Logic cannot be rationally accepted or rejected: we have no alar criteria for doing so. Still, it is there and seems quite self-evident to us all.

We need therefore to assume that logic (hand in hand with the common-sense theory of data validity) is the result of individual experiences from interactions with the environment. (Section 8 applies here, mutatis mutandis, of course.)

9.4. Beliefs and Belief Systems

We shall use the word "believe" in its common-sense and everyday meaning. We shall not postulate that "I believe that x" must imply "I do not believe that not-x". (This postulation will only be made for *scientific* – also called *rational* – beliefs (in section 10)).

(Relevant explicative literature: Ginsberg 1972. See also Hintikka 1962, and Schick 1970.)

9.4.1. Beliefs and Descriptive Statements

"To have a belief about x" means "to believe that x or that not-x". However, a concrete belief does not have this ambiguity as to what it concerns. A concrete belief assigns a concrete single validity value to a descriptive statement. (Similarly: Colby et al. 1969, a paper which has significantly biased our look upon beliefs, especially since we read Rescher 1964.) This descriptive statement will be said to be the *concern* of the belief.

9.4.2. The Truth Value of Beliefs

We shall say that a belief is true if it concerns a true proposition, false if it concerns a false one. Speaking generally, a belief has the same truth value as the proposition it concerns.

9.4.3. Belief Consistency

As soon as we have accepted the fundamental logical criterium "p and not-p cannot be simultaneously true in the same context", we can organize a belief system.

A *belief system* is a set of mutually logically consistent beliefs.

Two beliefs are mutually logically inconsistent if we can show that if we accept the one to be true, we cannot accept the other also to be true.

For instance, "Plato lived two thousand years ago" and "I met Plato at the movies last night" are mutually logically inconsistent. Consistency can be restored if we differentiate between a Plato who was a Greek philosopher two thousand years ago, and a friend nicknamed Plato.

Two beliefs p and q are logically consistent if the logical conjunction of p and q holds.

Not-p is merely the complement of p. We must never identify not-p with any *part* of the complement of p. For instance, it is incorrect to say that if p means "A is nervous", then not-p means "A is relaxed". On the other hand, it is correct to say that "A is relaxed" is *an instance* of not-p, and to conclude that "A is nervous" is inconsistent with "A is relaxed".

That p and not-p must not hold simultaneously seems to be a criterium of *fruitfulness* rather than of meaningfulness. "Not-p" means everything that is not p; do we not make this distinction between p and its complement in order to be able to say something relevant about p which we cannot say about not-p? Now if what we say about p also can be said about not-p, it is evidently of no use to make the distinction in the first place. Our strife for consistency in what we say must thus be seen as an endeavour to maintain distinctions. And distinctions are necessary for us since they enable us to structure our model of reality.

But it is wrong to conclude from "I like chocolate ice cream" that I dislike vanilla ice cream, or from "I dislike the intentions of party A" that I like the intentions of party B, even if there are only these two parties. It can namely happen that we from habit employ distinctions that can be fruitful to others or in other contexts although they in the present context are fruitless. It must therefore be a principle that superfluous distinctions do not as such cause inconsistencies in a set of beliefs.

9.4.4. Belief Coherence

However, consistency alone does not *warrant* that a belief system is more fruitful than any collection of assertions. A belief system can namely be logically consistent because it is *incoherent.*

Two beliefs are incoherent if the truth or falsity of one has no influence

upon the truth or falsity of the other. This is often the case when the beliefs concern different aspects of reality. For instance, "Plato lived two thousand years ago", "Plato has written philosophical studies", and "Socrates was a Chinese poet" are mutually incoherent beliefs; all three are therefore mutually logically consistent.

Two beliefs are (logically) *relevant* for each other it the truth value of at least one has direct or indirect influence ('logical consequence') upon the truth value of the other.

9.4.4a. Definition of "Theory"

A *theory* is a coherent belief system.

A belief system is *coherent* if all beliefs it contains are relevant for each other.

9.4.5. Theory Comprehensiveness

We all know that theories can be more or less misleading or adequate. A coherent belief system can namely happen to be logically consistent because it is *incomprehensive.*

We cannot demand that a belief system first be 'complete' before we are willing to judge its epistemic value. The point is that if we are to compare belief systems and to choose amongst them the most 'adequate' (in the sense of its being most reliable as a model of reality), then we must demand of an 'adequate' belief system that it includes beliefs concerning most of the phenomena that we, on the basis of everyday-life experiences, believe to be important. A coherent and consistent system of beliefs which simply ignores some major aspects of reality is obviously easier to achieve than a *comprehensive* one (that is one which includes beliefs on all major aspects of reality).

Merton (1967, p 39 ff) has argued that comprehensive theories regarding social reality cannot be constructed, and he has therefore advocated a so-called 'middle-range theorizing' approach. We can accept middle-range theorizing only as a provisional method since we, in opposition to Merton, believe that comprehensive social theories can be constructed.

9.4.6. Remarks

Rescher (1973), in an attempt to elaborate a coherence theory of truth, mentions (on p 169) comprehensiveness, consistency, and cohesiveness as three criteria to be used for assessing the truth of a set of propositions. (We recommend this book to those who wish to see these three concepts more extensively elaborated.)

Also Russell (1962, p 120, p 137) points out that consistency cannot be used to *define* truth, but it can be used as a *symptom* of truth.

9.5. What is Knowledge?

We say that a belief is *knowledge* if it is part of a comprehensive belief system and if the negation of this belief implies that essential parts of our 'world view' are negated. In other words, what we call knowledge is characterized by the fact that we cannot make the counter-belief fit into our 'world view'.

The 'world view' of a person is his most comprehensive theory of reality. (Alternatively, but not quite equivalently, we could stipulate that the 'world view' of a person is the theory of reality which he uses most frequently in everyday life.)

One can, of course, object to such subjectivistic definitions of knowledge, and instead demand that knowledge must be 'eternally' and 'objectively' true. We claim, however, in response to such criticism, that we can never be sure that we have *such* knowledge, and we would therefore never be permitted to call *anything* knowledge, because we could not prove it to be such knowledge. We prefer therefore a less restrictive definition, but envision that the thus defined concept also can be used if one speaks of 'collective world views' (such as 'the Christian world view' or 'the Marxist world view').

9.6. What is Science?

If we are presented with a set of beliefs, how can we tell whether it is scientific or not? We do make such judgments in reality, but what are our criteria? We are presumably very often mislead to call a set of beliefs scientific merely because it is presented in a certain form, by certain persons, in certain contexts, and/or because it concerns certain topics which we regard as scientific. This is not very surprising. Science is a system of 'rituals'; some of these rituals are pedagogical, they aim at conveying 'scientifically relevant information'; some rituals are heuristical, they aim at giving guidance whenever a scientist cannot rationally decide how to continue with his research; and some rituals are systematical, they aim at warranting the 'scientific soundness' of the results of scientific research if conducted according to the ritual (see Bunge 1967; Hage 1972; Leinfellner 1965; Rescher 1973; Stinchcombe 1968; Ziegler 1972).

We do not use the word "ritual" in a necessarily derogatory sense here, but rather in a 'deontic' one. Nor do we as yet wish to imply that a theory is scientific as soon as it has been developed according to scientific rituals. On the contrary, at least the choice of *systematical* rituals depends upon a

definition of "scientific theory" which is independent of our definition of "science".

Science has not only an epistemological/psychological but also socio-economical and political aspects. The definition of "science" can therefore vary with time and interests. But the definition of "scientific theory" ought to be more objective and more stable.

9.6.1. Paradigms and Rituals

Thomas Kuhn's 'paradigms' (cf. Kuhn 1962) can be said to be associated with sets of heuristical and systematical rituals. Paradigms are not themselves rituals, they are rather 'meta-rituals' that control or guide the selection or definition of rituals.

9.7. Theories and Descriptions

A single theory *describes* a single aspect of reality. It can therefore also be called a *description*. The meaning of a description is an aspect of reality, namely the described one.

The fact that a theory describes only a *single* aspect of reality follows from our assumption that a theory is a *coherent* belief system. If a set of propositions is found to be inconsistent, this can have its root in the involuntary mixing of fragments of descriptions of what according to our own assumptions must be regarded as *different* aspects of reality. For instance, if we somehow introduce the time dimension into a theory, we must not be surprised to find that parts of system-state descriptions are inconsistent if they refer to the system at different moments (cf. section 11).

9.8. Abstractions of Theories

Theories can, of course, concern more or less abstract features of reality. For instance, we may describe the everyday life in a Tunesian village, or we may theorize about measures based upon descriptions of interaction patterns in general. (Observe that we tend to use the word "description" to denote the accumulation of beliefs about everyday phenomena into a consistent whole, while we tend to use "theorizing" when it comes to assembling a consistent whole from propositions which we regard as denoting abstract entities or abstract states.) It is usual to say that a theory is ar abstraction of another theory, if the latter is an instantiation of the forme (or, equivalently, if the former is a generalization of the latter).

(In non-empirical science one says that the latter theory is a *model* of the former. Observe that this use of "model" is not in agreement with the use of the same word made here or in empirical science in general, unless one (not without justification) regards materialization as the ultimate instantiation of a theory.)

9.8.1. Theories and Metatheories

Theories can, of course, also concern classes of other theories. A theory which concerns other theories is called *metatheory*. You read thus a metatheory at present.

A metatheory is obviously an abstraction of the class of theories it concerns.

9.8.2. Abstraction, Formalization, Axiomatization

Formalization does not imply abstraction: a formalization is essentially a reformulation according to abbreviation conventions. *Axiomatization* means nowadays the specification of a set of constraining rules ('axioms') which delimit the properties and/or behavior of given objects ('eigenvariables') in a desired manner (Stegmüller 1958; Lorenzen 1962). The main advantage of axiomatization is the vast gain in the repertoire of entities one can theorize precisely about. (Similarly: Stachowiak 1973, p 24.) For instance, if we find it difficult to define what "I believe x" means, we can specify rules for the use of "I believe x" (cf. section 9.4, where we have done so) which constrain the use of this sentence and thus define the sentence meaning partially or completely.

Axiomatization and formalization together facilitate the detection of structural similarities in different theories. Such similarities suggest that one formulates an *abstraction* which is common to both theories. It is possible that one, in turn, detects similarities between the results of various abstractions, and thus can make second-order abstractions, and so on.

Is axiomatization possible in empirical science? The answer must be "evidently", since there exist several axiomatizations of mechanics (see Stachowiak 1973, p 261 for details), as well as an axiomatic biology (Woodger 1937; see also Carnap 1960). But these successful attempts at axiomatization of empirical-science theories are exceptions. (Stachowiak 1973, pp 261–264, contains a unique survey of the current state-of-affairs with respect to the axiomatization of empirical-science theories.)

If we compare an axiomatic formal system with an empirical-science theory, it strikes us that the formal system says nothing about the world, while the empirical-science theory is specific about the aspect of reality it concerns. The problem of axiomatization of an empirical-science theory

consists obviously of finding some set of axioms which can be said to be an abstraction of the whole theory, but when we have found one, we are at once dissatisfied with the vagueness (that is: generality) of that abstract version of the theory.

We will therefore next try to find new sets of axioms which not only define an overall structure (if the theory concerns a dynamic system: an overall structure of behavior), but which also define the involved elements better. Like this we continue until the set of axioms defines objects that no longer are abstractions of those in the original theory.

The reader will notice that such multi-step axiomatization and multi-levelled system description are essentially equivalent procedures.

(Other relevant literature: Frey 1967; Wilder 1956; Kyburg 1961.)

10. Consistency and Acceptability

The essential function of consistency tests is to decide for us whether we better *reject* a proposition or whether we are to *accept* it as an element in the belief system against which we tested it.

But what we hitherto have said about consistency seemed to be based upon a dichotomy of propositions into true ones and false ones. This is only correct if we interpret "true" as being synonymous with "acceptable", and "false" with "unacceptable", regardless of which criteria acceptance and rejection are based upon. We are all aware that propositions are not always clearly true or clearly false, but often merely half-true, nearly true, possibly true, probably true, hitherto true, sometimes true, or true in n cases out of m. Also our data are rarely completely reliable. Does this mean that logic is not applicable to such propositions and data, or that they even must be regarded as false since they are not completely true?

Well, common two-valued truth logic cannot accept less than perfect truths. But there are other logical calculi that can. Leinfellner (1965, p 33) proposes the following arithmetical truth functions instead of the common ly known ones:

conjunction (p,q) :=max[Z(p),Z(q)],
disjunction (p,q) :=min[Z(p),Z(q)],
implication (p,q) :=max[1,Z(q)-Z(p)+1],
negation (p):=m - Z(p) + 1,

where Z(p) and Z(q) are numerical values which can be thought of as measures of epistemic circumstances, while m is an assumed number of distinguishable measure results. (We call Z(p) *truth value of p* or *validity of p*.)

Two-valued truth logic is contained in this many-valued logic, but falsity is now not symbolized by "0" but by "2" (and in the general case by m). For instance, if p is false, then Z(p) =2, and negation (p):=2-2+1 =1. If q is true, then Z(q)=1, and negation (q):= 2-1+1 =2.

Many of the equivalences that hold in two-valued logic hold no longer in many-valued logics when m exceeds 2. For instance, DeMorgan's laws are no longer generally valid.

Lee (1971) has proposed a different many-valued logic (which he calls *fuzzy logic*) which symbolizes truth by "1" and falsity by "0", as is usual. Any number in the interval (0,1) can be used as an 'intermediate' truth

value.

A fuzzy logic can be defined as follows:
conjunction (p,q) :=min[Z(p),Z(q)],
disjunction (p,q) :=max[Z(p),Z(q)],
negation (p) := 1 - Z(p),
generalized-conjunction(P):=inf[Z(p)|p in P]
generalized-disjunction(P):= sup[Z(p)|p in P].

It is obvious that the total truth value of a belief system P is Z(P) := generalized-conjunction(P). But we can choose to set a lower limit L(P) for the truth values of beliefs which we admit into the belief system: the total truth value Z(P) can then never fall below L(P).

Now we approach an interesting point: Assume that L(P) is 0.3 and that p is a constituent of P, Z(p) being 0.4. Let q be the negation of p. Z(q) is then negation(p):=0.6, and q is thus acceptable to P although p and q should be inconsistent.

We notice that if L(P) exceeds 0.5, there are no inconsistency problems, and all the equivalences that allow interdefinability of the 16 possible operators in two-valued logic (for instance, DeMorgan's laws) are again valid. The reason for this is that although we use a fuzzy logic, there is a dichotomy into 'acceptable' (namely, if Z(p) equals or exceeds L(P)) or 'unacceptable' (if L(P) exceeds Z(p)). These two values can be interpreted as the truth values of a two-valued logic (of acceptability instead of truth) if they never can be consistent. This is guaranteed if L(P) exceeds 0.5.

We can therefore make the following stipulation: A theory is not *scientific* (or *rational*) unless each of its constituents has a validity that exceeds 0.5.

The positive definition of a scientific theory must certainly also mention comprehensiveness. We stipulate thus: A theory is *scientific* (or *rational*) iff it is comprehensive and each of its constituents has a validity that exceeds 0.5.

It is, of course, unfortunate that the meaning of "comprehensive" is vague. But since the 'complete' theory cannot be but indefinite in size, it is forever impossible to compute a meaningful ratio of the sizes of the constructed theory and the hypothetical 'complete' one. This does not exclude the possibility of other, more definite, explications of "comprehensiveness" in the future, although we at present cannot imagine one.

10.1. Remarks

(a) Rationally unacceptable beliefs can still have a heuristical function and suggest problems to be focused, differentiations to be made, studies to be

replicated, and so on.

(b) Popper (f.i., 1969, p 33 ff, especially p 39) stipulates that propositions or theories, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations.

Is this a stipulation that goes beyond ours? It seems to us that it does not. The stipulated requirement that a scientific theory of empirical phenomena must be based upon data with a certain minimal validity, appears to be stronger (besides being more explicit) than Popper's. Every element of a rational theory (in our sense) must obviously also be scientific according to Popper's falsification criterion.

10.2. Scientific Theory Construction

Let us now look at the process of scientific theory construction. Since we never begin from scratch, we can assume that we are given a pool of beliefs, some mutually consistent, some not. We wish to extract from this pool the set of beliefs which assembles into the most comprehensive and valid theory.

We must obviously begin by picking out all acceptable data; propositions cannot yet have any truth values. We then pick simple propositions: those that relate two data. We derive their truth values and decide whether they are acceptable. Then we pick propositions that relate three, four, five, etc. data, derive truth values, and accept or reject them. We pick thereafter propositions that relate two simple propositions. Et cetera. We assemble the theory successively.

By assuming a pool we make it clear that this process of theory construction does not in itself point out which propositions are to be made or which data collected. It is not a heuristic instrument.

Inconsistencies cannot arise in the assembled theory in the course of this procedure.

The procedure description given above is, of course, the *ideal* procedure of theory construction. Things are often somewhat different and more difficult in practise. We may, for instance, start with bits and pieces of theories what that we try to link. That means that the propositions in the pool in fact already do carry certain validity weights. And we may instead lack data and thus be unable to derive the theory in the manner described.

10.2.1. Assembling Theories From Inconsistent Fragments

What shall we do if data or propositions are found to be inconsistent?

The answer seems obvious. Our aim is to obtain a most comprehensive

and valid theory from the given pool elements. Given a set of data we must therefore first of all derive what Rescher (1973, p 78) calls 'maximal consistent subsets' of these data, namely all the subsets of the set of data which fulfil the following three conditions:

(a) the subset is non-empty,

(b) the subset if consistent,

(c) no non-member of the subset can be added to it without generating an inconsistency in it.

(Relevant literature: Rescher 1964; Rescher 1973; Swain 1970a. (There are no cross-references between Swain and Rescher.))

We shall add the following requirements to those for the selection of maximal consistent subsets, in order to obtain '*maximally comprehensive theory versions*':

(d) No acceptable data may be omitted in order to make certain propositions acceptable.

(e) When a maximal subset P_i has been defined, remove from it all data that are not linked with at least one other datum by a proposition in P_i.

10.2.1a. An Example

To illustrate with a very simple example:

If there are any data given to us, and we wish to include them in the theory P to be assembled, we must start with them, since the validity of propositions concerning relationships between data is to be computed by a truth function that has data validities as its arguments. Assume that we are given the following pool of propositions (and among them data), paired with their validity values:

$\{(p, 0.7),\ (r, 0.8),\ (p \Rightarrow q, 0.9),\ [(p \wedge q) \vee (\sim p \wedge \sim q), 0.6],$
$(\sim p \vee \sim r, 0.2),\ (\sim [(p \wedge q) \vee (\sim p \vee r)], 0.7)\}$.

We try to assemble maximal consistent subsets P_i of this pool:

Subset 1:

p exceeds $L(P) = 0.5$ and is P_1-accepted; r is P_1-acceptable; $p \wedge (p \Rightarrow q)$ implies (modus ponens) $Z(q) = 0.7$ and $(p \Rightarrow q)$ as well as q are P_1-acceptable; $[(p \wedge q) \vee (\sim p \wedge \sim q)]$ is P_1-acceptable; $(\sim p \vee \sim r)$ is not P_1-acceptable; $\sim [(p \wedge q) \vee (\sim p \vee r))$ can be reformulated (by double application of De-Morgan's laws) as $(\sim p \vee \sim q) \wedge (p \wedge \sim r)$ which is found to have validity 0.3 and therefore is not P_1-acceptable.

Finally, according to the connectivity rule (e) in section 10.2.1, remove r from P_1.

Subset 2:

Now we look once more at the rejected pool elements; all elements

accepted prior to the rejection must, as a whole, be inconsistent with each of the rejected elements, but taken one by one, some may still be acceptable.

Data that in isolation are unacceptable, however, are forever unacceptable in any subset P_1.

$(\sim p \vee \sim r)$ is P_2-acceptable if p or r are unacceptable. But such an assumption would violate the comprehensiveness rule d in section 10.2.1 and is thus out of the question. The same is true for the other rejected propositions.

We obtain thus a single maximal comprehensive theory version, P_1, with validity 0.7.

10.2.1b. Ranking Alternative Versions

If a choice is to be made between different maximal comprehensive theory versions, we can either use the total validities of each version as critical values, or we can multiply the total validity of each with the number of independent data in it, in order to roughly estimate relative comprehensiveness also.

10.2.1c. Remark

This model of scientific theory construction can be computerized. Does that mean that we can induce a computer to construct theories?

Not quite. This model is not a heuristic instrument. Tedious trial-and error search is the only method available to it for trying to elaborate a theory.

This model does thus not describe how researchers *in fact* procede when they theorize. It is, for instance, quite obviously as yet unrealistic to suppose that empirical theories are formalized. But nevertheless, it illustrates a rational approach to theory construction and can be used for orientation.

11. The Validity of Dynamic Models

It has now been shown to be relatively easy to assess the consistency of a description or of a belief system, because their components were assumed to be unchanging. But it is obvious that if we mix the descriptions of something at time t and of the same something at time w, and $t \neq w$, then we can expect the description mixture to be inconsistent. Dynamic systems change with time and we can therefore never *expect* to obtain consistent dynamic-system descriptions unless we either parametrize each description with an observation-time indicator (in that sense: McCarthy/Hayes 1969), or if we keep only a single 'current' description and erase all obsoleted description parts as soon as a change has occurred. Simulation models (and what we shall call *dynamic models* in general) are dynamic system descriptions for which the second solution method has been employed. What is called 'question answering systems' or 'information systems' predominantly employ the first (parametrizing) method if they at all are used for 'multi-state' descriptions.

11.1. The Necessity of Multi-Stage Validation

When we are concerned with the modelling of dynamic systems, however, we are usually not merely interested in proving the logical consistency of our description of the system, but also in assessing the empirical validity of the system model.

"To validate any kind of model ... means to prove the model to be true". (Naylor/Finger 1971, p 153. Cf. also Törnebohm/Radnitzky's (1971, p 54) conception of 'Abbildungstreue'.) But what is a 'true' model? The 'truth' we ask for must be more than logical truth in the sense of logical consistency of the very model specification. (Similarly: Reichenbach 1951, p 304.) It seems instead to be a hypothetical construct that can be explicated as follows: *If* we had a correct description of the model-relevant part of reality, and we thus could form a conjunction of the correct description with our own, then it would be possible to prove the truth or falsity of our own description (cf. section 8.2).

It is obvious that such a proof forever is out of the question and that we

only can try to approximate it somehow. Empirical data (that is: measures of reality) are generally accepted as being the least dubious approximations of the hypothetical True Description.

But the validity of data is, in fact, more dubious than empiricists and positivists tend to admit. The Cartesian approach to reality is not possible: Nothing can be recognized 'as such', that is: in isolation. Each 'measurement' is the valuation of a variable in an already constructed model, whether we, for instance, arrive at numbers or names by our 'measurement' depends on the model that we try to valuate (Karl K.K. Krarup, in discussion, 1973). Now, if measurements can be said to somehow achieve summarizing or simplifying descriptions of reality, then it is understanda ble why the meaning of data is model dependent. What we have said earlier (in section 1.9) about the necessity of knowledge regarding the context of discourse for the interpretation of an isolated sentence, is now seen to be valid also for data as measurement results. And we obtain futhermore a clue concerning how a common-sense theory of data validity (cf. section 8) is to be understood.

We can therefore choose to be more modest in our ambitions and stress the distinction between the modelling problem and the data-validity problem. We then simply presuppose that data are given to us and that we wish to design a model that is consistent with these data. This is the view taken by researchers who derive specific coefficients ('multipliers') from given data and then ask no more about the representativeness of these data. If new sets of data are regularly given, one simply 'puts the model back on the track' by computing new coefficients for the next period (for instance: Orcutt et al. 1961; also all macro-econometric models).

It is presumably a majority of model builders today who shares the view of Friedman (1953) who equates model validity with model-behavior validity. The latter is said to be assessed if one can prove the consistency of a normative description of the model's behavior with the description of the factual behavior of the model. The normative description simply defines all conditions that the built (or to-be-built) model is to fulfil with respect to behavior. The validity of the normative description must, of course, be assumed to be obvious.

Friedman's view seems to us to be too extreme, because it ignores the very model's validity. He reduces the model to a material implication: if the consequence is true, the material implication is true, even if the premises should happen to be false. (In content similar:Blaug 1962, p 612.)

Our criticism does not imply that we reject behavioral validity as a symptom of overall model validity. On the contrary. We merely do not wish to equate the two. (Cf. section 5.)

One can base a *behavioral-validity measure* upon comparative observations of model behavior: given the description of the true trajectory of a system during a certain period, we can measure the deviances of different model outputs from it during the same period. The model with the least deviant behavior is the behaviorally most valid.

Naylor and Finger (1971, p 156–158) have suggested a 'multistage validation' scheme as a 'solution' of the simulation-model validation problem. We find that it coincides with what we must suggest ourselves on the basis of our definition of composition models (given in section 5.2). That a simulation-model definition must be *internally consistent* is obvious by now. That the model's behavior, its consequences, must not fail to fulfil crucial conditions is also quite uncontroversial. The model must not behave in an obviously incorrect or unwanted manner. In short, the simulation model must be a *behavior model* of its prototype. (The prototype, in turn, must be understood to be a conception we have about a certain aspect of reality; the prototype is not reality itself unmediatedly. We cannot compare our simulation model with reality directly, but we can compare it with conceptions we have.) In order to be able to speak about a model's validity at all, to render the model at all more relevant, we *must* seek to make it become a *composition model* of its prototype. That *valid data* are necessary in a valid model follows finally from the fact that we can regard data as behavior models that are the elementary subsystems of the total model.

Can we somehow generalize the theory-assembly method described in section 10.2 such that it becomes applicable also to simulation models? Consistency within a simulation model can namely as yet only be assessed with regard to model states and static relationships ('structures'), but not with regard to functional relationships ('dependencies').

It appears at first that we cannot. For instance, we can add, to a given program, a subroutine which destroys the results computed by the rest of the program. This subroutine can be assumed to be syntactically correct (or at least sufficiently correct to be accepted by the computer). It is also executable, since its effects become manifest. The question is, of course: was it *intended* to have these destructive effects? To find out, we can test the consistency of a normative description of the (intended) effect with the description of the factual effects. Such a test of the subroutine's behavioral validity differs obviously from an internal-consistency test as proposed in chapter 10.)

We can preserve initial-description validity only if we retest the composition validity of a simulation model each time we tentatively have added new functions to it, before definitely accepting them; the tentatively added

functions must also be 'proven' valid separately in order to be definitely acceptable.

11.2. Model Validity and Common Sense

If we have a common-sense belief that x is the case, while science says that x is not the case, what shall we then believe? Once upon a time there was a common-sense belief that the earth was flat. There was also a scientific belief that atoms were indivisible. Science claims today that our typewriter consists of probability waves, while common sense tends to claim that it is rather more solid. Common sense is evidently no proof of truth, but neither is science infallible.

Science has common sense as its base of departure (cf. Nagel 1961, ch 1). Especially the behavioral and social sciences are highly dependent upon common-sense reasoning. Since the extent of their formalization is low, one cannot make deductions 'mechanically' according to simple syntactical rules; all inferences are instead semantically constrained. However, the ultimate criterium of validity when checking semantic inferences is common sense.

But also the valuation and the evaluation of completely formalized theories is ultimately guided by common sense; we can neither assign starting values nor interpret the theory's consequences without having an idea about the context in which the theory is to be, or has been, applied. We can thus never quite avoid the necessity of relying on common sense, but formalization can move the borders of our dependence. However, if we can *not* formalize our models, we ought to take the bull by the horns and *explicitly* incorporate as much as possible of our common-sense reasoning into them.

We see thus that we try to 'guarantee' the ('a priori') validity of a model by pointing to the common-sense conceptions which have led to the model's basic assumptions and starting data, and that we seek to 'check' its ('a posteriori') validity by comparing the conclusions obtained from it with our common-sense world view.

Now it can happen that this 'check' reveals that the model lacks 'a posteriori' validity, although it has 'a priori' validity. We assume at once that something is wrong with the model, that there is a logical error (a 'bug') in it. If we become convinced that there is none such, we must check the 'a priori' validity conditions once more in extra detail (cf. *modus tollens*). If even these prove immaculate, we are forced to revise and differentiate certain of our common-sense conceptions (cf. *modus ponens*).

Of course, instead of making *direct* reference to common sense, we can also base our validity judgements upon comparisons with, or the results of, other (already validated) models.

11.2.1. Triviality versus Heuristics

It is sometimes said that common sense is trivial while science is 'genial'. We tend to believe the opposite; let us therefore discuss the matter.

Logic constrains science. It permits only a limited set of types of inference steps, and each step is subject to specific application rules and preconditions. As long as one accepts these imposed restrictions, one is guaranteed 'sound reasoning'. That is: each permitted inference step can be regarded as a process of 'transmitting reliability' from a premise to a conclusion; the rules of logic warrant that we will never be confronted with a contradiction.

As an illustration of this let us draw an analogy to the game of chess. Also there the set of elementary moves is constrained. If you break against the rules, you are out of the game. In science, if you encounter a contradiction, you also are 'out of the game'. Everyone knows that it is not easy to predict the consequence of a move in chess, and even less simple to organize one's moves in such a way that it leads to a fruitful end. Not even computers with their immense enumerative capacities solve this task easily. Botvinnik (1970), the Russian chess champion, has demonstrated that efficient game playing depends on the player's capacity to structure and thereby reduce the search space, and he has tried to formulate a computerizable heuristic strategy for such structuring which resembles his own style of reasoning.

Since the 'board' of science is much larger than that of chess, it is even more difficult to predict long-term consequences of a single move, and therefore also to organize research. An enumerative approach to science is obviously out of the question, and we *must* rely on heuristic strategy. The structure we seek will give us a kind of *Vorverstehung* or *frame of reference* which is to guide our continued action. It seems to us that 'common sense' is the label given to the frame of reference (or the *set* of frames of reference) most commonly used in every-day life and in science.

Common sense is thus not irrelevant for science nor is it trivial. In fact, it is easier to claim that *science* is trivial if its heuristic aspect is ignored: it is then simply an unintelligent (although quite logical), planless, enumerative producing of next moves.

Is a sequence of trivial moves itself trivial? In retrospect, yes, even if the generation of that sequence once was a non-trivial, heuristic task. A completed game, or a fully developed scientific theory, is in that sense trivial.

This implies that the so-called 'inexact' sciences in fact are less trivial than the 'exact' ones (or, rather, than the 'exact' parts of the 'exact' sciences).

11.3. Practical Consequence: What is the Use of Running Before You Know the Direction?

Analytical and macro-level models are computationally more effective than micro-level and composition models. They are also usually more concise and more easy to formulate. But when the complexity of analytical models is of such an order that common sense fails to enable us to assess their 'a posteriori' validities (cf. Forrester 1971, p 15), we have no other way of validating them than by comparing them with composition models of at least the same order of complexity.

The validation of composition models is easier than that of analytical models, because we can decompose a composition model into submodels which we can validate one by one, in a hierarchically ascending manner. This implies that we, in the composition model case, know what we deal with; an analytical model, in contrast, is a 'black box': it is not directly visible whether its internal structure makes sense.

Analytical models are usually used for planning. This makes us pose the question: Shall we accept the contemporary trend which gives higher priority to computational efficiency than to model validity? Of course not. We must first of all guarantee that what these models suggest is to our own best, according to *our own* criteria.

An effective social science is a potentially very dangerous threat to us all, the more marked the more it becomes a black box which only a few experts know how to manipulate. The only weapon against Big Brother 'Black Box' seems to be its being checked by generally accessible composition models.

12. Simulation and Simulation Languages

We shall assume the word "simulation" to be synonymous with "computer simulation" and employ it for denoting the use of a computerized simulation model. A 'computerized simulation model' (or, for short, a 'simulation model'), in turn, is a model (we shall assume that it is dynamic) which has been formulated in some (usually formal) language which a computer can interpret. (We presuppose a computer only because it is 'stupid' enough to force us to be explicit in our assumptions.) A simulation model needs not be formulated in any of the common 'simulation languages' such as SIMSCRIPT, SIMULA, GASP, GPSS, CSMP, DYNAMO, or CSL; any language will do if the computer only can be made to interpret it. That means that you, in principle, can start to formulate a simulation model without knowing any programming language or anything about computers; the only thing you in that case need to observe is that you never refer to anything which is not part of the description of the model. (For instance, you may refer to the *word string* "the transcendentality of aesthetic experience" but you may *never* refer to the *meaning* of that word string unless you have explicitly provided it. The computer would namely otherwise not understand what you refer to. It knows only signs.)

(An excellent survey of simulation-modelling languages and simulation-modelling concepts is found in Kiviat 1971. Cf. also Emshoff/Sisson 1970; Reitman 1971; Dutton/Briggs 1971; Wyman 1970.)

The formulated model is a theory. In order to derive conclusions from it, we need first to valuate the variables which are not assigned a value within the theory itself, and we can then derive the values of all the variables in the program step by step. The 'conclusions' are represented by the values of certain of these variables when certain conditions are fulfilled.

For instance, the following *could* be a simulation-model formulation:

(1) Assume a person called I.
(2) I is located at position A.
(3) I makes a random step.
(4) The number of steps, denoted by "N", is now 1.
(5) I makes another random step.
(6) N has now increased by 1.
(7) If I is at A, I stops. Otherwise continue with statement 5.

I is here simply a variable which has the value A. The concrete value, for

instance 'e5', must be given initially. It is the 'input' to the model. Other tacit inputs are given via a random-move function which somehow assigns a new value to I. The conclusion or 'output' of the model is to be the value of N when I stops. We can let I start several times from A and eventually compute the average number of random steps needed for returning to A. Of course, there is a chance that I never returns to A, and the 'computation' will go on 'forever'.

The use of an existing, flexible, and problem-oriented simulation-programming language ('SPL') can quite essentially facilitate simulation modelling, or even model descriptions in general. Kiviat (1971, p 411) is even willing to claim that this is the most important reason for using SPLs. We agree with him. The design of an own, original SPL may seem simple, but it is extremly difficult to make it easy to read, to understand, to use, to modify, and to extend. The simpler a programming language is for the user, the more difficult is it to implement the interpreting program for that language.

And yet, there is reason for giving it a try if the processes one wishes to simulate have quite particular structures that cannot be easily formulated and referred to in existing SPLs. (Cf. Volume 3.)

12.1. Time and the Simulation-System Structure

The most important reason for using simulations is time (cf. section 11), and time is also a central concept in simulation modelling. How time is advanced can be solved in many different ways (cf. Emshoff/Sisson 1970, p 159–169; Kiviat 1971, p 417–425; Wyman 1970, p 91–98), but the choice of time-advancement procedure depends upon the complexity of the inter-relationships between various subprocesses of the total process. If many subprocesses interact heavily with one another, and especially if the triggerings of these subprocesses are not temporally fixed but determined by outcomes of other subprocesses, it is namely difficult to or impossible to derive a queue order among the subprocesses once and for all in the beginning of a simulation. It is then instead necessary to search through all subprocess definitions and to check each whether it is to be active or passive during the present simulated time period. Combined event-queue and process-triggering designs for the next-subprocess selection exist (for instance, in SIMULA, but also in the Atman Monitor described in Volume 3).

12.2. System States

The definition of a simulation model comprises the specification of an initial system state. Subsequent system states will all be modifications of the temporally immediately preceding system states. These modifications are brought about by one, several, or all of the subprocesses in the system that constitute the total process (that is: the system's behavior).

The initial system-state specification must comprise the listing of all entities assumed to exist during the initial period of simulated time. Each entity must be characterized by properties; these properties define the entity's individuality and either correspond to behavior-differentiating parameters in the subprocess definitions, or are simulation-output variables.

12.3. Operating Rules

The simulation model's behavior is defined in terms of subprocesses (functions), some of which may be unconditionally applicable to all entities while others are only conditionally applicable. More specifically: applicability can be made to depend upon the current properties of certain system components.

12.4. Remark

We do not wish to go into technical details concerning simulation modelling in this text, no matter how important these can be for the understanding of what simulation modelling is in practice. (Such details are separately discussed in Volume 3, and most books on simulation modelling specialize on technicalities.) We shall instead continue to focus upon the epistemological status of simulation modelling and show that it is the only approach available to empirical scientists who wish to work with large dynamic causal models.

12.5. Simulation versus Question-Answering

It became apparent in section 11 that the introduction of the time dimension into system descriptions leaves us with two representation choices: We can either parametrize each proposition with respect to time, or we can let time implicitly always be *now*, which requires that propositions are im-

mediately deleted when they are invalidated.

There are no established names for the two different approaches to dynamic-system descriptions, but we shall call them 'the class of symbolic approaches' and 'the class of simulation approaches'. (Let McCarthy's 'Advice Taker' (1959) be a representative for the first class, Newell/Simon's GPS (1961) a representative for the second. (These representatives have been suggested by Simon/Siklóssy 1972, p 329.))

Symbolic approaches employ modal calculi for the description of 'world states' (see Carnap (1967) for a pre-computer attempt of this kind) and for the mechanizable derivation of consequent 'world states' under hypothetical sequences of applications of the various defined rules. The problem-solving process is thus translated into a process of finding a sequence of rule applications which will transform the description of the present 'world state' into the supplied description of that 'world state' which is to be regarded as the goal state.

A number of efficient symbolic-approach problem-solvers (often called 'question-answering programs') have been developed during recent years: f.i., REF/ARF by Fikes (1971), QA3 by Green (1961), PCDB by Sandewall (1971). But we cannot simply go and use these problem solvers: we must define a comprehensive data base of deduction rules and facts, before we can make other than trivial use of these problem solvers. No one has, as yet, defined a data base for general use which contains a vast number of common-sense deduction rules and basic facts about our world although all symbolic-approach problem-solvers require such data in addition to problem-specific rules and facts.

Simulation approaches do not differ much from the symbolic ones in the representation of rules and facts; this difference is often exaggerated. Syntactically, there is little difference between "put X Y Z" and "has X Y Z". Semantically, the effect of having written "has X Y Z" is the explicit storage of "has X Y Z" in the data base; the effect of having written "put X Y Z" is the ordered storage of the value Z under the indicator Y on the property-list of X; the phrase "put X Y Z" or the term "X Y Z" are not explicitly stored.

The essential difference between the two approaches lies in the effects of rule applications. In symbolic-approach problem-solvers, applications of a rule to a description will produce a new description without removing the old one. In simulation-approach problem-solvers, application of a rule in a given state will produce a new state which *replaces* the old one.

The advantage of the simulation approach over the symbolic one is obvious: the number of states of a simulation model is at any time exactly *one*, while the number of state descriptions stored in the data base tends to

increase with each rule application. But this advantage implies simultaneously a disadvantage: Once we have applied a rule in a simulation model, we cannot recover the state that was. The symbolic-approach problem-sover, in contrast, is capable of 'hypothetical reasoning'; it can try out alternative sequences of rule applications because the original description is never removed or replaced.

If a simulation model is very small, it is, of course, possible to explicitly save *copies* of the model in specified states, which make it possible later to abandon the model when a sequence of rule applications has put it into an undesirable and/or unpromising state, and then to *restart* with a copy of the model as it was before. Unfortunately, this is a clumsy solution for more than very few copies if the model is large. However, there *are* possibilities to implement 'hypothetical-reasoning ability' in large-scale models also. Such features are called 'backtracking mechanisms'. (Cf. Hewitt 1970; Newell/Simon 1972, p 814 ff; IJCAI73, p 677 ff.)

Does the disadvantage overweigh the advantage of the simulation approach over the other one? How important is it to be able to perform hypothetical reasoning?

Hypothetical reasoning is the reason why we have science, in the first place. It allows us to foresee the consequences of our actions or of inaction and to seek to avoid undesirable and unpromising future circumstances while there is still time to look for alternative courses of action and to counterbalance unfavorable trends.

Hypothetical reasoning is often called *planning*.

In simulated individuals ('robots') as problem-solvers, the purpose of using hypothetical reasoning is analogical. The robot is to be able to 'regulate' *before* unfavorable circumstances are an unavoidable reality. (Thus, he is not to behave like a thermostat, because thermostats begin their regulatory action first when an 'error' (that is: a deviance outside the norm range) already has occurred.)

The symbolic approach seems 'smarter' on the surface: All you need to do is to submit small bits of information in any order you like, and the data-base monitor takes care of the ordering. But when it comes to specifying behavior rules, we soon find that we have gained nothing: We must still be very explicit about preconditions and consequences, exactly as we need to be when we write a computer program. As a matter of fact, eventually we even find that writing a program is easier because of better means of referencing variables and of controlling the flow of computation.

Besides, the symbolic approach implies essentially harder problems regarding the artificial problem-solver's 'understanding' of what the reasoning is about (see, f.i., Davidsson/Harman 1971; or

Binkley/Bronaugh/Marras 1971, as examples of discussions of attempts at deriving a 'meaning' from a given sentence); a simulation approach problem-solver, in contrast, comprises a current model state which is the very meaning of what we speak about when we speak about facts, and the computational effects of the 'evaluation' of instructions or rules upon this model state are the very meaning of these instructions or rules.

In summary: Both approaches have advantages over the other. But simulation is the only good choice when it comes to modelling large composed systems over a long period of time. symbolic-approach systems 'explode' in these cases because too much information is stored.

But syntheses of the two approaches become more and more frequent (cf. IJCAI73, especially p 677 ff, but also p 255–277) and promise to allow us to balance the disadvantages, while simultaneously enabling more concise and more easily modifiable information representations. The simulation-model aspect of such syntheses is called 'semantic database' (similarly: Pople 1972, p 342; cf. also IJCAI73, p 356 ff).

12.6. Micro- versus Macro-Simulations

When we simulate life processes, we distinguish certain represented entities and call them '*decision units*' if we choose to let the total system's behavior be essentially determined by functions that have the properties of such entities as their arguments.

We speak of *micro-simulations* if individuals (on a given description level) are the only decision units. For instance, if cells are among the decision units in a biological simulation, a cell-biologist will call it a micro simulation.

A *macro-simulation* is a simulation where only aggregates of individuals are decision units. The simulation of two-person interaction would be regarded as a micro-simulation study by a sociologist, but as a macro-simulation by a cell-biologist and possibly also by a psychologist (if he regards psychic processes as the individuals of his abstraction level).

Macro-level decision units (or, for short, 'macro units') are derived from micro-level decision units ('micro units') by abstraction. This implies a loss of information, but it reduces thereby also the computer-storage capacity needed for simulation. Macro-simulation model design starts therefore logically with the choice of the variables one cannot disregard; the macro-level decision units are defined as the elements of the Cartesian product of these variables. (Macro-level decision units will also be called *categories*.)

Macro-level decision-unit behavior is usually defined in terms of transi-

tion probabilities or flow rates (cf. Orcutt et al. 1961, p 33; Coleman 1964; Forrester 1968; Norlén 1972). These can be explicitly specified for each decision unit, or be embedded in a general function that computes a 'decision' on the basis of the values which define the decision unit within the Cartesian product set.

The categories can be assigned weights which indicate the sizes of the sets of micro level decision units from which they have been derived by abstraction. These weights are called *category sizes* (and micro-level decision units from which a category has been abstracted are said to 'belong to' the category). Category sizes increase and/or decrease in consequence of the net of inflows and outflows according to the given flow rates per time period.

12.7. Multi-Levelled Simulations

We call a simulation (or a simulation model) multi-levelled if micro units as well as macro units are decision units in the simulation model.

It is in practice difficult and probably impossible to achieve complete system resolutions from given levels to others. (The possibility of total or infinite resolution is a different problem which we shall not be concerned with here.) We must account for this fact; our models can therefore not be completely reductionistic—they must be multilevelled.

Assume, for example, that we wish to simulate a firm. We describe the formal and informal social structures that we have observed, describe even the personalities of certain key individuals ("gate keepers"), describe the production processes within the firm, and we outline the environment. We will find that we have described the organization in terms of departments, sections, groups, etc., and we have simultaneously specified *a few but not all* details within these. Thus, we cannot simply resolve a group into a system of individuals. We must instead carry out our simulation on several levels: We must use our information on details for computing the next-level (more macroscopic) phenomena; next-level data, on the other hand, can be used as cues or frames on the lower (more microscopic) level.

12.8. The Choice of Decision Units

One does not choose macro-simulations if one can avoid them. But they are often unavoidable, the most common reason being that micro-simulations of the same total systems would involve such immense

numbers of decision units that no available computer could store all information about them. Other reasons are lack of knowledge regarding interdependencies among micro-level decision units, or concerns about efficiency in terms of computation time required for a simulation.

Micro-simulations count amongst their advantages intuitiveness, that is, a 'face validity', through increased structural similarity of the model with its prototype, and the freedom not to use a mathematical model (or, more specifically, a mathematically formulated flow model adopted from physics) but to employ non numerical properties and relationships if one so wishes. Non-linear and discontinous relationships can be modelled easily (cf. the example in Orcutt et al. 1961, p 11–12).

The choice of simulation type is implicit in the choice of decision units. The choice of decision units, in turn, must take the mentioned advantages and limitations into account and look at the modelling intentions. The most severe disadvantage of the use of categories (macro-units) instead of individuals (micro-units) is the loss of the individualities of category members; they can therefore not be made to accumulate any individual experiences and commitments.

We see at present only a single 'solution' to this often significant problem, namely to interpret categories as 'representatives' that have been sampled from all micro-units (cf. Orcutt et al. 1961, p 285–296). Such a 'solution' is a trade-off of generality against detail. (As we detail the definition of the category, the category size will tend to decrease; and the smaller the category size, the fewer micro-units can any member drawn from that category be a representative for.)

It is not legitimate to generalize the behavior of the representative directly to the behavior of all n members of the category it represents. For instance, in a simulation study of Iceland, if n=10000 and the representative gets married one day, we cannot conclude that all n category members get married on the same day. (The choice of time-step length is obviously constrained if we wish to be able to make such generalizations. We need, for instance, look up how long time it can be expected to take until n Icelanders that belong to this specific category have become married. All rates must, of course, be synchronized with that which requires the longest time steps.)

A 'representative solution' of this kind seems not to be adequate for achieving multi-levelled simulations in general, because of the limitations upon time-step lengths that follow from a 'one-for-all' interpretation, unless we have relatively small categores. A splitting up of one category into several smaller ones, all with own individualities as representatives, can achieve a neutral balance in the distorting effects of random factors that

co-determine the timing of actions by representatives. The significant regularities will instead become more evident.

12.8.1. Aggregates As Representatives

Large causal models can most often be decomposed into many relatively closed subsystems consisting of elements or of small aggregates of elements. We can therefore solve the decision-unit choice problem quite adequately for most large causal models as follows:

We simulate a statistically satisfactory sample of subsystems in full detail, one after one. These subsystems are then regarded as representatives or 'prototypes' for all other subsystems.

For instance, assume that a rural area comprises n villages. Compose a number m (which is less than n) of 'prototype' villages by micro-simulation, and sample then all remaining n-m villages from these m (drawing with replacements, of course). Thereafter apply the operating rules for villages to the n villages and obtain thus a rural-area characterization which again is micro-computed (although on a higher level than before).

This procedure can, of course, be used recursively to characterize more comprehensive subsystems and eventually the total system. Let thus r rural areas be independently synthesized as described. These can then obviously be taken as 'prototypes' for many other rural areas, and in an interplay with similarly derived models of urban areas can all be used to derive the characteristics of a whole region.

The essential idea behind this procedure is first of all the fact that random deviations from statistically normal values tend to be compensated within aggregates and over time, and aggregates can therefore more safely than individuals be made representatives of others. The second rationale is that no aggregate needs to be made representative of a large number of other ones; there will in most cases be much fewer aggregates than individuals. This second rationale makes hierarchical total-system composition especially valuable.

12.9. Composition Models as Prototypes for Behavior Models

The more detailed the models become, the less surveillable becomes their behavior. Verbal theories cannot become equally detailed, they would much earlier become unsurveillable. Theories are 'portable' orientation devices for individuals, sufficient to help them understand the surface processes of their everyday lives, while simulation models of some ade-

quacy are computer-bound (and thus 'non-portable') because of their complexity.

Bonini (according to Dutton/Starbuck 1971, p 4) has pointed to the 'paradox' that very realistic causal-structural simulation models tend to become almost as unsurveillable as the prototype system. This is correct, although simulation models are far more transparent than reality. We can trace every single causal step in a simulation, but it is the sheer mass of all causal steps that beats us. Nevertheless, causal-structure simulation models can be used as 'wind tunnels' for simpler models. They are at least for that purpose of practical epistemological importance.

We have several times in this text pointed out that since we can never have a True Description of Reality, we cannot prove a model's validity simply by proving that it has been derived according to the principles described in chapter 5. But assume that we have a very detailed and comprehensive deterministic model of reality which few doubt to be highly valid: it would then be possible to derive a multitude of valid simplifications rather easily. And there is, above all, reason to hope that those who derive simplifications remain better aware of what they have omitted than they do now when they 'simplify reality' directly.

It must surely be justified to implement such a detailed deterministic 'standard' model in a scale that exceeds all that we hitherto have dreamt of being able to realize all in one piece. To design and run a 'standard' model will, or course, incur costs that will be many times that of various simplifications. Many simplifications are used for planning; if they should eventually prove invalid, the consequent 'repair costs' are usually not computable. Cheap planning models are perhaps very costly in a wider perspective.

12.10. The Format of Reports

What are the proper results of simulations? We said earlier that the 'conclusions' of a model simply are the values of certain variables at specified moments. The question is thus "which variables?" and "which moments?"

The easiest answer is "all variables and all moments". But that is out of the question when we are concerned with large model simulations with many time steps. We must thus make summaries in the form of transition matrices or frequency tables or structure tables (abstractions of transition matrices), and/or restrict our interest to the values of certain 'symptomatic' variables at sampled moments.

Three approaches to the solution of this significant problem can be distinguished:

(a) *Step-by-step summaries*, which give reports in the form of a system-history outline; focusing must be allowed upon specific individuals, allowing the user to obtain life-trajectory descriptions for each of these individuals.

(b) *Dynamic graph displays* of the moves of the simulated elementary entities in the common property space. This solution requires an interactive computer system with graphic displays. As yet can this solution not be general because such systems are not generally available. Besides, since the user can look at the reports only as the simulation is run, and then only at one 'slice' of the property space at a time, this approach must be reserved for special-purpose simulations.

(c) *Selective surveys*, which list the distributions of properties in certain systems at a given point of time. Cross-classifications may be asked for, statistical characteristics may be computed, etc. Socalled *panel studies* can easily be derived from sets of surveys on the same system made at different time points.

We can build result analyses into the very model and thus obtain compressed data. The complete repertoire of statistics is at our disposal since our 'simulated data' can always be made to be of optimal quality with regard to precision, completeness, measurement reliability and validity, sample size and re-observability (without 'measurement' after-effects).

But the user of simulated data can also analyze these exactly as he would process empirical data. The simulated data can be given the form he is used to and trained to work with; he can, for instance, submit them to separate data-analysis programs of his own choice.

Step-by-step reports seem to be of relevance especially for case-study oriented users.

13. The Simulation Modelling of Knowledge Accumulation

Let us now introduce the (somewhat difficult to pronounce) concept '*jnana system*' ("jnana" means "insight" in Sanskrit) to denote computer simulation systems that incrementally (although not necessarily automatically) model a group of life scientists as they go about trying to model life phenomena. This means that a jnana system models processes of scientific discovery as well as the result of such processes.

Observe that new insights in, for instance, cognitive psychology, artificial intelligence, or the sociology of knowledge, enter into a jnana system on two levels: on the scientist-simulation level and on the general life-modelling level. A jnana system can thus come to embody an operational epistemology for the life sciences. (This booklet is our own first small step into that direction. Cf. also Stachowiak 1973.)

13.1. The Cybernetics of Sociocybernetics

True research is always a regulation process: A problem is to be solved somehow, but the method of solution is as yet unknown. This implies interaction between obtained intermediate results and the researcher's research strategies and tactics. Effective control of the research process cannot be guaranteed in pioneering research where prototype solutions are lacking.

Today one can still not say that the problems which sociologists have been trying to master for the last century are at least well defined or well structured. Effective research is definitely improbable if one is without guidance from a well-structured global model of the problem matter. Individual research efforts will not be compatible in design and therefore not give complementary results that can be accumulated. Research may be focused (in the global perspective) on trivial problems while (globally seen) crucial questions never are posed.

But one cannot expect that a first model is forever adequate. Cybernetical research is itself cybernetic in that it is explicitly problem adaptive, not dogmatic. (This distinction corresponds to the differentiation

between research as a regulation process and research as a steered process.) The cybernetical research process is to produce a sequence of increasingly elaborated (and hopefully also increasingly adequate and operational) models. If there are several model sequences under development, then they are regarded as eventually converging subsequences of a single sequence.

From this follows that the triviality of initial models in a sequence tells nothing about the adequacy or complexity of later models. This explicit step-by-step development from triviality to complexity is also favorable from the pedagogical point of view, especially since concrete multi levelled simulation models are difficult to explain and to grasp if presented all at once.

The most simple models can furthermore be illustrative instances of a number of abstract cybernetical principles such as feedback control of behavior, adaptiveness, multistability, ultrastability, goalseeking; several are also proof for the fact that the interplay of a few simple system parts can result in system-behavior patterns which are very difficult to predict.

It is a common misconception that the imposition of constraints necessarily narrows the output range of a system; exactly the opposite is obviously the case when one elaborates a system by specifying conditional properties.

The cybernetical research process can itself be modelled as an adaptive system, with a given initial repertoire of behavior rules, existing in a vague task environment. The models it produces can be assumed not to be outputs into the environment but rather elaborations of the very system structure. The research process is then finally neatly conceived of as a problem solving and question-answering system which actively seeks to improve its capabilities.

As yet, at least, the system-structure elaboration is mainly performed by us human researchers, but the increasing efforts put into artificial intelligence research promise to automatize parts of that work also. In any case, the system structure is laid down in the form of a computer program. The system's internal state, upon which it bases its behavior, is the bulk of experience and data stored away in the computer-memory devices.

It is easy to underestimate the importance of this possibility of interpreting the socio cybernetical research process as a cybernetic 'artificially intelligent' system. This conception implies namely that there is no longer any distinction between theory and methodology in sociocybernetics: there is no longer any essential difference between the simulating and the simulated system. Advances in artificial intelligence, f.i., are of relevance both to the simulating system trying to solve its problems and to the

simulated systems as they try to solve *their* problems in their simulated environment.

13.2. Automatic Heuristics

The whole jnana system is a computerized research aid, but it is not completely automatic. It is designed for interaction with one or several researchers. Still, many parts on many levels of the system *are* automatic; they initiate processes and carry them on without explicit human orders or guidance.

We shall now discuss two epistemologically especially interesting automatically theory-improving features in the jnana system design.

The jnana system's default model of social reality (and of its parts) is actually a large database. (Let us denote it in this section by "A" for ease of reference.) Simulations do not influence this database directly; the simulation model is (in the initial phase of each separate run) *generated* by A as a dynamic model.

A is to be regarded as an artificial 'belief system'. The jnana system receives data from its users, but it does not accept them uncritically. In principle is every datum associated with a record which states source of information, date of observation, and reliability estimates or criteria, as well as a 'plausibility weight', computed jnana-system internally to indicate how often the datum participated in a simulation leading, according to the user's feedback message, to a plausible result. An increase or decrease of this weight occurs also implicitly if the system finds that a 'finer' (that is: more detailed) simulation of a certain subprocess gives a result which was independently postulated on a higher description level. Logical consistency tests can be based upon these plausibility weights.

We must try to 'educate' the jnana system with much and varied and reliable information to start with, we must 'reward' it when it produces plausible results, and we must indicate possible error factors if a simulation result was implausible.

It may seem that we always would obtain educator-biased systems. That is true, but this needs in practice not become any handicap if we let *several different* educators train the system, and if *data have been entered at different levels of detail*. A theory, in order to be preferred by the jnana system, must be more plausible than alternative theories on *all* levels of detail. *Observe* that the jnana system synthesizes its very own most plausible theory from the database. This 'most plausible theory' is not strictly delimited but rather, in game-theoretical terms, the result of a mixed sampl

ing strategy from the database: The more plausible a datum or hypothesis is found to be in comparison to competitive contrary or even contradictory data or rules, the more often is it expected to be chosen. To be more precise: the expected proportion of occasions on which a datum or rule is chosen among competitors is in direct proportion to its relative plausibility among these competitors.

This conception of 'most plausible theory' is in analogy to automata theoretical argumentation concerning the problem of constructing reliable automata from unreliable components (cf. Cowan 1966). By the way, if the simulation model is to *generate*, not to *predict*, behavior, is a *mixed* sampling strategy better than a non-probabilistic, permanent choice of the single absolutely most plausible item and permanent rejection of all less plausible alternatives.

14. References

We have chosen only to include referenced literature in the following list and not turn it into a bibliography. These references do not give justice to those authors who have influenced us most profoundly, because we, so to speak, have absorbed and dissolved them into our own style of thinking to such a degree that single references no longer can suffice. We think here especially of Ashby's *An Introduction to Cybernetics*, 1956 and March/Simon, *Organizations*, 1958. Bocheński's *Précis de logique mathématique*, 1949, had influenced our ambitions with respect to explicitness already before we encountered the book by Ashby in January 1962, in the course of trying to compile a verbal life-trajectory model.

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Sociocybernetics

is a way to model social reality, and man in particular. It is an integrating interdisciplinary approach which explicitly acknowledges the fact that scientific capacity has been essentially augmented through the advent of the computer as an easily accessible tool. Sociocybernetics can be said to be a modern and operational epistemology for the social and behavioral sciences.

Homo Cyberneticus

is an exemplification of how sociocybernetics is to be used for comprehensive causal modelling. *Homo Cyberneticus* seeks to outline a coherent map of everyday life, upon which computer simulations of social-interaction processes can be based. It can also be looked upon as a research program that can systematize and unify research efforts in otherwise quite dissimilar fields, for instance psychology of religion and robotology, linguistics and ergonomics, or literature and futurology. It seeks to thus reassemble man into one whole.

Homo Cyberneticus comprises 5 booklets:

Volume 1: The Simulation of Everyday Life: A Synopsis
Volume 2: Towards an Epistemology of Dynamic Causal Modelling
Volume 3: Heuristic Simulation Programming
Volume 4: Artificial Psychology and Generative Micro-Sociology
Volume 5: A Macro-Sociological Framework